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An Account of Observations made in Scotland on the Distribution of the Magnetic Intensity. By James Dunlop, Esq. Communicated by Sir T. M. Brisbane, K. C. B.

(*Read 19th April* 1830.)

Makerstoun, Kelso, 15th March 1830.

My Dear Sir,

Accompanying I beg leave to transmit you Mr Dunlop's paper on the distribution of the magnetic intensity over this country, which I formerly mentioned when I had the pleasure of seeing you, and which I request you will lay before the Royal Society.

Mr Dunlop has so fully and clearly detailed his mode of proceeding with these observations, that it would be unnecessary for me to enlarge upon them further, than to state, that, in point of number, extent of country, combined with precaution, accuracy, and consistency, I consider they are unrivalled in this or any other country.

While on this subject, it might not be considered as irrelevant in me to express the regret I feel that this important branch of science should have been so much neglected in Great Britain; and I am induced to make the remark, from the valuable assistance I obtained from Professor Hansteen's magnetic chart, in my voyage to New South Wales and home; as, by comparing the position of the ship, inferred by his magnetic curve, it was found to agree, in an extraordinary manner, with the true place on board, whilst the dead reckoning was erroneous to the extent

of many degrees. I can, therefore, have no hesitation in declaring it as my opinion, that a ship may be navigated with infinitely more accuracy by one of Professor Hansteen's magnetic charts, than by dead reckoning, and is so perfectly simple, that the merest novice having one of these charts, has only to determine the variation on board his ship, and with the latitude to examine the place on the chart of the intersection of the magnetic curve, which shews at once the position of the ship.

I trust I may yet see the time when Mr Dunlop's observations may be emulated by others, both in this and other countries, which, as they extend the bounds of human knowledge, may be considered a great benefit to man.

I remain, my dear Sir, yours most truly,

Th. Makdougall Brisbane.

John Robison, Esq. Secretary Royal Society, &c.

To Lieut.-Gen. Sir Thomas Makdougall Brisbane, K. C. B.

Sir,

I have now the honour of presenting to you the result of my experiments with the magnetic apparatus belonging to the Royal Society of Edinburgh, for the loan of which I feel greatly indebted to the kindness of Dr Brewster, and to you in particular, as the expenses of the journey were liberally defrayed by yourself.

I trust the experiments will be worthy of your acceptance, as they are the most extensive series of any which have yet been made in Britain; and, in selecting a situation where to make the experiments, I was very careful to avoid the immediate vici-

nity of buildings, or any object which I might suspect would influence their accuracy. I regret not having a portable dipping needle to accompany the horizontal, by which means the whole of the magnetic intensity at the different stations could have been obtained.

The apparatus consists of a tripod stand, a box, a thermometer divided to REAUMUR's scale, and two needles. In the bottom of the box there is a circle on paper, graduated into degrees, over the centre of which the needle is suspended by a silk fibre of about four inches in length. The box is also furnished with foot-screws for the proper adjustment of the apparatus.

Previous to the commencement of the observations, the suspended needle was drawn about 25 degrees out of its natural position, and allowed to diminish the arc of its vibration to 20 degrees, before I commenced registering the observations. The needle, by which the other was drawn aside, was always placed at a distance of 15 or 20 yards from the apparatus while the observations were making; and I was always very careful to divest myself of every thing of iron during the experiments.

With regard to Professor HANSTEEN's method of observing, described in his letter to Dr BREWSTER, which accompanied the apparatus, "to take the mean of 300 vibrations, commencing with an arc of 20 degrees, and ending with an arc of 2 degrees;" this I found impossible for me at all times to do, for, as I advanced north on my journey, the horizontal intensity of the earth's magnetism was diminishing, and the needles, particularly cylinder No. I., would have almost been in a state of rest before I had completed 300 vibrations. I was therefore under the necessity of increasing the arc as I required in going north, and of decreasing it again as I came south. This circumstance caused me to make experiments, to ascertain a correction to be applied to the increased arc of vibration.

As Professor HANSTEEN, in the letter before alluded to, has

given his own observations, made in Christiania with these needles, in the year 1827, previous to their being sent to Edinburgh, these observations I have here given, because I consider them connected with mine, as illustrative of the difference of the magnetic intensity between Christiania and Edinburgh as station points. The time of 300 vibrations is reduced to the temperature of $+ 13°.3$ REAUMUR, by the formula $T = T' [1 - 0.0003711 (t' - t)]$, supposing the number of vibrations, at the mean temperature t, to be T seconds, and at the temperature t', to be T′ seconds.

I have accompanied this paper with a table, containing the time which cylinder No. I. required to make 300 vibrations, reduced to Professor HANSTEEN's standard cylinder by DOLLOND, the last column of which contains the horizontal intensity at the different stations, that of Edinburgh being taken as unity, and also a chart, on which the time of 300 vibrations of the needle are annexed to the name of the station where the observations were made.

The lines of equal intensity, or what Professor HANSTEEN calls the *isodinamical magnetic lines*, are laid down to every five seconds of time; and I have also copied in *dotted lines* from HANSTEEN's chart (lately published), the isodinamical lines extended over Scotland, from his observations in Norway, Sweden, and Denmark.

It will be seen by comparing these lines, that there is a difference of about 10 seconds in the time of 300 vibrations, between the lines copied from HANSTEEN's chart, and those deduced from my observations. From this it would appear that the horizontal intensity through Scotland is less than Professor HANSTEEN's experiments led him to expect. The experiments which I made in Edinburgh, at the back of Captain BASIL HALL's house, No. 8. St Colme Street, gave nearly the same result with those made behind Coates Crescent by Professor OER-

PLATE I.

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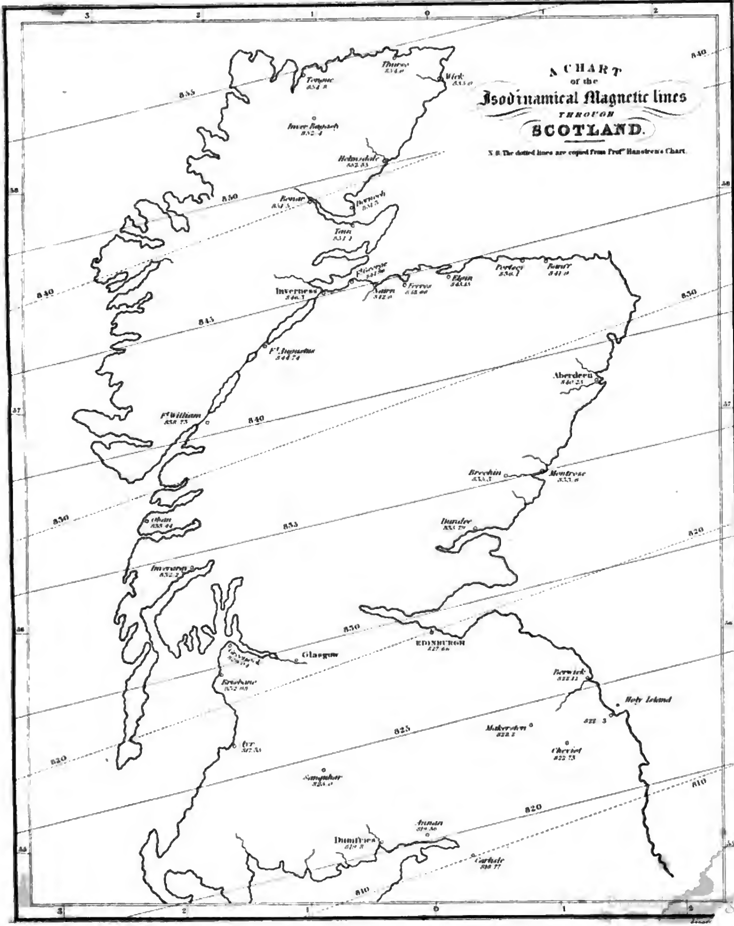

STED, in the year 1823. But at the time I was making my experiments, I considered them objectionable, from the great quantities of iron in the vicinity, used as pipes for the conveyance of water, railing, &c. I mentioned this to Captain HALL and Professor WALLACE, who were present at the experiments, and afterwards to Mr ADIE, who kindly requested me to make my experiments in his field at Canaan Cottage, where such objections could not apply. I accordingly did so, in company with Professor WALLACE and Captain ROBERTSON, R. N., and found a difference of about 10 seconds in the time of the needle making 300 vibrations at the respective stations, which is nearly the amount of the difference between Professor HANSTEEN'S lines and mine.

I should not omit to mention, that the needles have always been kept separate, and in their respective cases, and that their magnetism has not been disturbed since they came into my possession.—I am, Sir, your humble servant,

JAMES DUNLOP.

OBSERVATIONS WITH THE NEEDLES IN CHRISTIANIA,

MADE BY

Professor HANSTEEN, for the purpose of comparing them with his Standard Cylinder by DOLLOND, previous to their being sent to Edinburgh.

The Observations were made in Professor HANSTEEN's Garden.

FLAT NEEDLE. First Arc 20°.

1827, June 24. P. M. Chronometer gaining + 8″·2 daily.				Ther. + 17°·2 R. + 15·9; Mean + 16·55
No.	Time.	No.	Time.	Time of 300 vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 43 46·6	300	7 0 8·8	16 22·2
6	44 6·4	306	28·4	22·0
12	26·0	312	48·0	22·0
18	45·8	318	1 7·6	21·8
24	45 5·6	324	27·2	21·6
30	25·2	330	46·8	21·6
36	44·8	336	2 6·4	21·6
42	46 4·6	342	26·0	21·4
48	24·4	348	45·6	21·2
54	44·0	354	3 5·2	21·2
60	47 3·6	360	24·9	21·3
Corr. for Temp. = −1·18 s.			Mean =	16 21·63

CYLINDER, No. 1. First Arc 20°.

1827, June 24. P. M. Chronometer gaining + 8″·2 daily.				Ther. + 14°·5 R. + 12·1; Mean + 13·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	9 5 28·4	300	9 18 49·2	13 20·8
10	55·2	310	19 15·6	20·4
20	6 22·1	320	42·4	20·3
30	49·0	330	20 9·2	20·2
40	7 15·6	340	35·6	20·0
50	42·4	350	21 2·4	20·0
60	8 9·2	360	29·0	19·8
70	35·8			
80	9 2·5		Mean	13 20·21
90	29·2			= 800·21
100	55·9		Rate of Chron.	− 0·06
			Time of 300 Vibs. =	800·15

OBSERVATIONS IN CHRISTIANIA,—*continued.*

Standard Cylinder by Dollond. First Arc 20°.

1827, June 27. A. M. Chronometer gaining + 8″·2 daily.				Ther. + 17°·6 R. + 17·3 Mean + 17·45
No	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	10 30 16·0	300	10 43 58·0	13 42·0
10	43·4	310	44 25·6	42·2
20	31 11·0	320	52·8	41·8
30	38·4	330	45 20·3	41·9
40	32 6·0	340	47·6	41·6
50	33·6	350	46 15·2	41·6
60	33 1·0	360	42·4	41·4
70	28·4			
80	55·8		Mean	13 41·79
90	34 23.2		=	821·79
100	50·6		Rate of Chron. —	0·06
			Time of 300 Vibs. =	821·73

Cylinder, No. I. First Arc 20°.

1827, June 27. A. M. Chronometer gaining + 8″·2 daily.				Ther. + 17°·4 R. + 17·9 Mean + 17·65
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	10 58 59·6	300	11 12 23·6	13 24·0
10	59 26·4	310	50·4	24·0
20	53·4	320	13 17·3	23·9
30	11 0 20·4	330	44·0	23·6
40	47·2	340	14 4·2	24·0
50	1 14·0	350	37·6	23·6
60	40·9	360	15 4·4	23·5
70	2 7·6			
80	34·4		Mean	13 23·8
90	3 1·4		=	803·80
100	28·2		Rate of Chron. —	0·06
			Time of 300 Vibs. =	803·74

Standard Cylinder by Dollond. First Arc 20°.

1827, June 27. P. M. Chronometer gaining + 8″·2 daily.				Ther. + 19°·0 R. + 18·3 Mean + 18·65
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 20 31·6	300	7 34 11·8	13 40·2
6	48·0	306	28·1	40·1
12	21 4·5	312	44·4	39·9
18	21·2	318	35 0·8	39·6
24	37·6	324	17·2	39·6
30	54·0	330	33·6	39·6
36	22 10·4	336	50·0	39·6
42	26·8	342	36 6·0	39·2
48	43·2	348	22·4	39·2
54	59·6	354	38·8	39·2
60	23 16·2	360	55·2	39·0
			Mean	13 39·56
			=	819·56
	Rate of Chronometer		—	0·06
	Time of 300 Vibrations		=	819·50

Cylinder, No. I. First Arc 20°.

1827, June 27. P. M. Chronometer gaining + 8″·2 daily.				Ther. + 19°·1 R. + 19·2 Mean + 19·15
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 57 56.8	300	7 11 18·6	13 21·8
6	58 12·8	306	34·4	21·6
12	28·8	312	50·5	21·7
18	44·9	318	12 6·4	21·5
24	59 1·0	324	22·4	21·4
30	17·1	330	38·4	21·3
36	33·0	336	54·4	21·4
42	49·2	342	13 10·2	21·0
48	7 0 5·2	348	26·0	20·8
54	21·2	354	42·0	20·8
60	37·2	360	58·4	21.2
			Mean	13 21.32
			=	801.32
	Rate of Chronometer		—	0·06
	Time of 300 Vibrations		=	801·26

OBSERVATIONS IN CHRISTIANIA,—*continued.*

CYLINDER, No. I.

1827.	Hour.	Ther.	Time of 300 Vibs.	Red. to + 13°.3	Red. Time of Vibs.	Hour.	Therm.	Time of 300 Vibs.	Red. to + 13°·3	Red. Time of Vibs.
June 27	H. M. 11 7 A.M.	+ 17°·65	803″.74	— 1″·30	802″·44	H. M. 7 6 P.M.	+ 19°·2	801″·26	— 1″·77	799″·49
				STANDARD CYLINDER BY DOLLOND.						
June 27	H. M. 10 38 A.M.	+ 17°·45	821″·73	— 1″·26	820″·47	H. M. 7 29 P.M.	+ 18°·7	819″·50	— 1″·64	817″·86

June 27. No. I. at 11 A. M. = 802″·44, at 7 P. M. = 799·49 } = 800″·965

Daily variation = 2·95

Standard DOLLOND at 10½ A. M. 820″·47, at 7½ P. M. 817·86 } = 819″·165

Daily variation = 2·61

Accordingly, the Reduction of No. I. to HANSTEEN's Standard, which he has used in the observations through Norway, Sweden, Denmark and Finland, and a part of Germany, is $= \frac{819''\cdot165}{800''\cdot965} = 1\cdot02272$, and the log. Red. = + 0·0097578.

OBSERVATIONS AT MAKERSTOUN.

The Experiments were made in the open air, about 60 yards from the Observatory.

CYLINDER, No. I.

1829, January 20. P. M. Chronometer losing — 4″ daily. Ther. + 2°·3 R., + 2·0; Mean + 2·15

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 2 13 28·0	300	H. M. S. 2 26 49·5	M. S. 13 21·5
10	55·2	310	27 15·5	20·3
20	14 21·6	320	42·0	20·4
30	49·0	330	28 9·1	20·1
40	15 15·5	340	36·0	20·5
50	42·4	350	29 2·5	20·1
60	16 9·0	360	29·5	20·5
70	35·8			
80	17 3·0		Mean	13 20·486
90	29·0			= 800·486
100	56·3		Rate of Chron.	+ 0·033
			Time of 300 Vibs. =	800·52

CYLINDER, No. I.

1829, January 20. P. M. Chronometer losing 4″·0 daily. Ther. + 2°·0 R., + 2·7; + 2·35

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 2 57 45·1	300	H. M. S. 3 11 6·2	M. S. 13 21·1
10	58 11·2	310	32·0	20·8
20	48·2	320	58·7	20·5
30	59 5·0	330	12 25·3	20·3
40	32·0	340	52·8	20·8
50	59·2	350	13 20·8	20·6
60	3 0 25·2	360	45·8	20·6
70	53·3			
80	1 18·3		Mean	13 20·67
90	45·7			= 800·67
100	2 12·0		Rate of Chron.	+ 0·03
			Time of 300 Vibs. =	800·70

OBSERVATIONS AT MAKERSTOUN,—*continued.*

Cylinder, No. I.

1830, January 23. P. M. Chronometer going Mean Time.				Ther. + 2°·9 + 2·9 Mean + 2·9
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 2 15·0	300	2 15 36·5	13 21·5
10	42·0	310	16 3·0	21·0
20	3 8·5	320	30·0	21·5
30	35·7	330	56·0	20·3
40	4 2·0	340	17 22·8	20·8
50	29·2	350	49·3	20·1
60	55·5	360	18 15·6	20·1
70	5 22·8			
80	49·0		Mean	13 20·76
90	6 16·2		Time of 300 Vibs. =	800·76
100	42·2			

Cylinder, No. I.

1830, January 23. P. M. Chronometer going Mean Time.				Ther. + 2°·3 + 2·3 Mean + 2·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 41 44·8	300	2 55 6·0	13 21·2
10	42 11·8	310	32·6	20·8
20	38·8	320	59·5	20·7
30	43 5·2	330	56 26·3	21·1
40	32·2	340	52·5	20·3
50	58·8	350	57 19·0	20·2
60	44 26·0	360	46·0	20·0
70	52·5			
80	45 19·0		Mean	13 20·61
90	46·0		Time of 300 Vibs. =	800·61
100	46 12·8			

Cylinder, No. I.

1829, June 28. P. M. Chronometer gaining + 2"·1 daily.				Ther. + 10°·7 + 10·1 Mean + 10·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 48 3·5	300	7 1 26·0	13 22·5
10	29·8	310	52·0	22·2
20	56·8	320	2 19·0	22·2
30	49 24·0	330	45·5	21·5
40	50·5	340	3 12·5	22·0
50	50 17·0	350	39·0	22·0
60	44·0	360	4 6·0	22·0
70	51 10·5			
80	37·5		Mean	13 22·06
90	52 4·5		=	802·06
100	31·5		Rate of Chron. —	0·015
			Time of 300 Vibs. =	802·045

Cylinder, No I.

1829, June 28. P. M. Chronometer gaining + 2"·1 daily.				Ther. + 10°·1 + 10·0 Mean + 10·05
No.	Time.	No	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 7 47·5	300	7 21 10·2	13 22·7
10	8 14·8	310	37·0	22·2
20	41·0	320	22 3·5	22·5
30	9 8·0	330	30·0	22·0
40	35·0	340	57·0	22·0
50	10 1·5	350	23 23·5	22·0
60	28·5	360	51·0	22·5
70	55·5			
80	11 22·5		Mean	13 22·27
90	49·0		=	802·27
100	12 16·0		Rate of Chron. —	0·015
			Time of 300 Vibs. =	802·255

OBSERVATIONS AT MAKERSTOUN,—*continued.*

CYLINDER, No. I.

1829, June 29. A. M. Chronometer gaining + 2″·0 daily.				Ther. + 14°·5 + 14·7 Mean + 14·6
No.	Time.	No.	Time.	Time of 300 Vibrations.
	h. m. s.		h. m. s.	m. s.
0	9 29 2·3	300	9 43 28·0	13 25·7
10	29·5	310	54·7	25·2
20	55·5	320	44 21·3	25·8
30	30 23·0	330	48·3	25·3
40	49·7	340	45 15·3	25·6
50	31 16·8	350	42·0	25·2
60	43·8	360	46 8·5	24·7
70	32 11·0			
80	37·5		Mean	13 25·36
90	33 4·3			= 805·36
100	31·5		Rate of Chron.	— 0·015
			Time of 300 Vibs. =	805·345

CYLINDER, No. I.

1829, June 29. A. M. Chronometer gaining + 2″·0 daily.				Ther. + 14°·7 + 15·0 Mean + 14·85
No.	Time.	No.	Time.	Time of 300 Vibrations.
	h. m. s.		h. m. s.	m. s.
0	9 50 32·5	300	10 3 58·3	13 25·8
10	59·0	310	4 25·0	26·0
20	51 26·2	320	52·2	26·0
30	53·3	330	5 18·8	25·5
40	52 20·8	340	45·7	24·9
50	47·8	350	6 12·5	24·7
60	53 14·7	360	39·2	24·5
70	41·5			
80	54 8·2		Mean	13 25·34
90	35·2			= 805·34
100	55 1·8		Rate of Chron.	— 0·015
			Time of 300 Vibs. =	805·325

FLAT NEEDLE.

1828, December 1. A. M. Chronometer E. losing 12ˢ. daily.				Ther. + 4°·0 + 4·0 Mean + 4·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	h. m. s.		h. m. s.	m. s.
0	11 40 11·2	300	11 57 8·2	16 57·0
10	45·3	310	42·0	56·7
20	41 19·4	320	58 15·6	56·2
30	53·5	330	49·7	56·2
40	42 27·0	340	59 22·7	55·7
50	43 1·5	350	56·8	55·3
60	35·2	360	12 0 31·0	55·8
70	44 9·4	370	1 5·0	55·6
80	43·3	380	39·0	55·7
90	45 17·1	390	2 12·8	55·7
100	51·3	400	46·8	55·5
Mean		1015″·945	=	16 55·945
Rate of Chron.	=	+ 141		
Time of 300 Vibs. =		1016·086		

FLAT NEEDLE.

1830, February 14. P. M. Chronometer I. going Mean Time.				Ther. + 5°·0 + 3·4 Mean + 4·2
No.	Time.	No.	Time.	Time of 300 Vibrations.
	h. m. s.		h. m. s.	m. s.
0	2 48 45·0	300	3 6 2·5	17 17·5
10	49 20·0	310	37·0	17·0
20	44·7	320	7 11·5	16·8
30	50 30·0	330	46·0	16·0
40	51 4·3	340	8 20·5	16·2
50	39·0	350	55·0	16·0
60	52 13·3	360	9 30·0	16·7
70	48·0	370	10 4·0	16·0
80	53 22·6	380	38·5	15·9
90	57.2	390	11 12·8	15·6
100	54 32·4	400	48·0	15·6
Mean		1036″·30	=	17 16·30
Rate of Chron.		0·00		
Time of 300 Vibs. =		1036·30		

OBSERVATIONS AT MAKERSTOUN,—*continued.*

FLAT NEEDLE.

1829, June 29. A. M. Rate of Chronometer gaining 2″·5.				Ther. + 14°·0 + 16·5 Mean + 15·25
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	10 40 21·8	300	10 57 29·0	17 7·2
10	56·5	310	58 3·0	7·5
20	41 30·8	320	37.0	6·2
30	42 4·8	330	59 11·0	6·2
40	39·7	340	45·5	5·8
50	43 13·8	350	11 0 19·5	5·7
60	47·8	360	53·5	5·7
70	44 22·4	370	1 28·3	5·9
80	56·5	380	2 1·8	5·3
90	45 31·0	390	36·5	5·5
100	46 5·0	400	3 10·0	5·0

Mean 1026″·00 = 17 6·0
Rate of Chron. = —0·00

Time of 300 Vibs. = 1025·97

FLAT NEEDLE.

1829, June 29. P. M. Chronometer gaining 2″·5 daily.				Ther. + 14°·0 + 10·8 Mean + 12·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 6 21·6	300	8 23 26·2	17 4·6
10	56·0	310	24 0·5	4·5
20	7 30·3	320	34.8	4·5
30	8 4·4	330	25 9·0	4·6
40	39·7	340	43·0	3·3
50	9 13·5	350	26 17·0	3·5
60	47·5	360	51·0	3·5
70	10 21·6	370	27 25·5	3·9
80	56·0	380	59·8	3·8
90	11 30·0	390	28 33·7	3·7
100	12 4·4	400	29 7·5	3·1

Mean 1023″·91 = 17 3·91
Rate of Chron. = — 0·03

Time of 300 Vibs. = 1023·88

The Observations at Makerstoun corrected for temperature, and the time of 300 vibrations reduced to that of Professor HANSTEEN's Standard Cylinder by DOLLOND.

CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to 13°·3 R.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829.	H. M.	°	″	″	″	″
Jan. 20.	2 21 P. M.	+ 2·15	800·52	+ 3·312	803·832	822·10
—	3 6 P. M.	+ 2·35	800·70	+ 3·253	803·953	822·22
1830.						
Jan. 23.	2 10 P. M.	+ 2·9	800·76	+ 3·090	803·850	822·11
—	2 50 P. M.	+ 2·3	800·61	+ 3·268	803·878	822·14
1829.						
June 28.	6 56 P. M.	+ 10·4	802·045	+ 0·863	802·908	821·15
—	7 16 P. M.	+ 10·05	802·255	+ 0·967	803·242	821·49
June 29.	9 37 A. M.	+ 14.6	805·345	— 0·388	804·957	823·25
—	9 59 A. M.	+ 14.85	805·325	— 0·463	804·862	823·15

OBSERVATIONS AT MAKERSTOUN,—*continued.*

1829, January 20. at 2 h. 43 m. P. M. 300 vibrations, reduced to HANSTEEN'S standard, Mean			822·16 s.
1830, January 23. at 2 30 P. M. 300 vibrations,		=	822·12
		Mean	822·14
1829, June 28. at 7 6 P. M. 300 vibrations,	= 821·32 s.		
29. at 9 48 A. M. 300 vibrations,	= 823·20	Mean	822·26
Daily variation,	= 1·88	Diff. =	·12

FLAT NEEDLE.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3 R.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN'S Standard.
1828. Dec. 1.	11 H. 51 M. A. M.	+ 4°·0	1016″·086	+ 3″·506	1019″·59	″
1830. Feb. 14.	3 0 P. M.	+ 4·2	1036·30	+ 3·502	1039·80	
1829. June 29.	10 51 A. M.	+ 15·25	1025·97	— 0·742	1025·23	
—	8 17 P. M.	+ 12·4	1023·88	+ 0·34	1024·22	

By comparing the results contained in the above table, it appears that the magnetism of the flat needle is not stationary. Indeed, Professor HANSTEEN found it so, and expressed himself to this effect, in a letter to Dr BREWSTER, which accompanied the apparatus, that " he suspected the magnetism in the flat needle had not yet become stationary, and that he had accompanied the apparatus with a needle of his own (Cylinder No. I.), which had been used in several expeditions, and could be more depended upon."

		No. I. Cylinder.		Flat Needle.
In Christiania, 1827, June 24, Time of 300 vibrations,	=	800· seconds	=	980· seconds
At Makerstoun, 1829, June 28, Time of 300 vibrations,	=	802·	=	1024·
Difference between the places by both needles,	=	2· seconds	=	44· seconds

Assuming the magnetism of No. I. to have remained stationary, the flat needle has, in two years, increased its time in making 300 vibrations, no less than 42 seconds of time; and the following deductions will shew the decrease of its magnetic force to be very unequable, proportionate to the time elapsed:

		Time.	Diff.	Ann. Increase.
1827, June 24. (Reduced to Makerstoun)	300 vib. flat needle,	= 982·5 seconds		
			= 37·1	= 26 seconds
1828, Dec. 1. At Makerstoun,	300 vib.	= 1019·6		
			= 5·1	= 9
1829, June 29. ...	300 vib.	= 1024·7		
			= 15·1	= 24
1830, Feb. 14. ...	300 vib.	= 1039·8		

OBSERVATIONS AT EDINBURGH.

The following Observations were made in the back Garden adjoining No. 8. St Colme Street, on the very spot where Captain Basil Hall made his observations with Captain Sabine's apparatus in the year 1827; but from the great quantity of iron-railing in the immediate vicinity, I suspect the present observations may be affected by it.

Cylinder, No. I. First Arc 20°.

1829, July 8. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 11°·0 + 11·2 Mean + 11·1
No	Time.	No.	Time.	Time of 300 Vibrations.
0	H. 8 M. 31 S. 9·8	300	H. 8 M. 44 S. 29·0	M. 13 S. 19·2
10	37·0	310	55·5	18·5
20	32 3·5	320	45 22·0	18·5
30	30·4	330	48·5	18·1
40	57·0	340	46 15·6	18·6
50	33 23·7	350	41·5	17·8
60	50·4	360	47 8·0	17·6
70	34 16·8			
80	43·5		Mean	13 18·33
90	35 10·6			= 798·33
100	37·0	Rate of Chron.		— ·018
		Time of 300 Vibs.		= 798·312

1829, July 8. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 11°·2 + 11·6 Mean + 11·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. 8 M. 50 S. 15·4	300	H. 9 M. 3 S. 34·5	M. 13 S. 19·1
10	42·0	310	4 0·8	18·8
20	51 8·8	320	27·0	18·2
30	35·0	330	53·4	18·4
40	52 2·0	340	5 20·0	18·0
50	28·7	350	46·3	17·6
60	55·5	360	6 12·7	17·2
70	53 22·3			
80	49·0		Mean	13 18·186
90	54 15·8			= 798·186
100	42·5	Rate of Chron.		— ·018
		Time of 300 Vibs.		= 798·168

OBSERVATIONS AT EDINBURGH,—Same Place,—*continued.*

FLAT NEEDLE. First Arc 20°.

1829, July 8. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 11°·6 + 11·6 Mean + 11·6
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	9 19 30·0	300	9 36 23·5	16 53·5
10	20 3·6	310	57·0	53·4
20	38·0	320	37 31·2	53·2
30	21 12·7	330	38 5·0	52·3
40	46·5	340	39·4	52·9
50	22 20·2	350	39 12·8	52·6
60	54·6	360	47·0	52·4
70	23 28·3	370	40 20·4	52·1
80	24 2·2	380	53·8	51·6
90	36·8	390	41 28·0	51·2
100	25 10·4	400	42 2·3	51·9
			Mean	16 52·46
				= 1012·46
			Rate of Chronometer,	— ·02
			Time of making 300 Vibrations,	= 1012·44

CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to 13°.3 R.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829. July 8.	H. M. 8 39 A. M.	+ 11°·1	798″·312	+ 0°·652	798″·964	817″·12
—	8 58 A. M.	+ 11·4	798·168	+ 0·562	798·73	816·88
FLAT NEEDLE.						
—	H. M. 9 31 A. M.	+ 11°·6	1012″·44	+ 0°·64	1013″·08	

OBSERVATIONS AT CANAAN COTTAGE, near Edinburgh.

The following Observations were made at Canaan Cottage, in the centre of a Field belonging to Mr Adie, where no suspicion of error caused by the presence of iron can be presumed.

Cylinder, No. I. First Arc 20°.

1829, July 8. P. M. Chronometer gaining + 2''·5 daily.				Ther. + 13°·6, + 15·0; Mean + 14·3
No.	Time.	No.	Time.	Time of 300 vibrations.
	H. M. S.		H. M. S.	M. S.
0	1 3 45·5	300	1 17 16·0	13 30·5
10	4 13·3	310	42·5	29·3
20	40·3	320	18 10·0	29·7
30	5 7·0	330	36·5	29·5
40	34·4	340	19 3·4	29·0
50	6 1·2	350	31·0	29·8
60	28·5	360	57·6	29·1
70	55·5			
80	7 22·5	Mean	=	13 29·557
90	49·3		=	809·557
100	8 16·6	Rate of Chron.	—	·018
		Time of 300 Vibs.	=	809·539

Cylinder, No. I. First Arc 20°.

1829, July 8. P. M. Chronometer gaining + 2''·5 daily.				Ther. + 14°·0, + 13·4; Mean + 13·7
No.	Time.	No.	Time.	Time of 300 vibrations.
	H. M. S.		H. M. S.	M. S.
0	1 35 10·3	300	1 48 40·0	13 29·7
10	37·6	310	49 6·3	28·7
20	36 4·6	320	34·0	29·4
30	31·2	330	50 0·0	28·8
40	58·2	340	27·5	29·3
50	37 26·0	350	54·0	28·0
60	52·5	360	51 21·5	29·0
70	38 20·0			
80	46·8	Mean	=	13 28·986
90	39 13·8		=	808·986
100	41·0	Rate of Chron.	—	·018
		Time of 300 Vibs.	=	808·968

Cylinder, No. I. First Arc 20°.

1829, July 8. P. M. Chronometer gaining + 2''·5 daily.				Ther. + 13°·3, + 12·4; Mean + 12·85
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	1 58 36·0	300	2 12 5·0	13 29·0
10	59 2·8	310	31·8	29·0
20	30·3	320	58·8	28·5
30	56·8	330	13 26·2	29·4
40	2 0 24·0	340	52·5	28·5
50	51·0	350	14 20·0	29·0
60	1 18·3	360	46·8	28·5
70	45·5			
80	2 12·0	Mean		13 28·843
90	39·5		=	808·843
100	3 6·0	Rate of Chron.	—	·018
		Time of 300 Vibs.	=	808·825

Cylinder, No. I. First Arc 20°.

1829, July 8. P. M. Chronometer gaining + 2''·5 daily.				Ther. + 10°·8, + 10·4; Mean + 10·6
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 58 27·0	300	3 11 56·0	13 29·0
10	54·2	310	12 23·0	28·8
20	59 21·2	320	49·8	28·6
30	48·8	330	13 16·8	28·0
40	3 0 15·7	340	43·5	27·8
50	42·0	350	14 10·3	28·3
60	1 9·3	360	38·0	28·7
70	37·0			
80	2 3·0	Mean		13 28·457
90	30·8		=	808·457
100	57·2	Rate of Chron.	—	·017
		Time of 300 Vibs.	=	808·440

OBSERVATIONS AT CANAAN COTTAGE,—*continued.*

Flat Needle. First Arc 20°.

1829, July 9. A. M. Chronometer gaining + 2″·5 daily.
Ther. + 12°·5, + 13·0; Mean + 12·75

No.	Time. h. m. s.	No.	Time. h. m. s.	Time of 300 Vibrations. m. s.
0	10 6 45·0	300	10 24 0·0	17 15·0
10	7 19·6	310	35·0	15·4
20	54·5	320	25 8·5	14·0
30	8 29·0	330	43·6	14·6
40	9 3·2	340	26 18·0	14·8
50	38·0	350	52·2	14·2
60	10 12·6	360	27 26·8	14·2
70	47·2	370	28 1·2	14·0
80	11 22·3	380	35·8	13·5
90	56·5	390	29 10·0	13·5
100	12 31·2	400	44·8	13·3
			Mean	17 14·227
				= 1034·227
Rate of Chronometer,				— ·029
Time of 300 Vibrations,				= 1034·200

Flat Needle. First Arc 20°.

1829, July 9. A. M. Chronometer gaining + 2″·5 daily.
Ther. + 13°·0, + 15·0; Mean + 14·0

No.	Time. h. m. s.	No.	Time. h. m. s.	Time of 300 Vibrations. m. s.
0	10 35 47·8	300	10 53 3·4	17 15·6
10	36 22·5	310	37·8	15·3
20	57·0	320	54 12·4	15·4
30	37 32·0	330	46·7	14·7
40	38 6·2	340	55 21·2	15·0
50	41·0	350	55·4	14·4
60	39 15·5	360	56 30·2	14·7
70	50·2	370	57 4·8	14·6
80	40 25·0	380	39·3	14·3
90	59·2	390	58 13·4	14·2
100	41 34·0	400	48·0	14·0
			Mean	17 14·745
				= 1034·745
Rate of Chronometer,				— ·029
Time of 300 Vibrations,				= 1034·716

Same Place.

Cylinder, No. I. First Arc 20°.

1829, July 9. A. M. Chronometer gaining + 2″·5 daily.
Ther. + 15°·0, + 17·0; Mean + 16·0

No.	Time. h. m. s.	No.	Time. h. m. s.	Time of 300 Vibrations m. s.
0	11 6 49·5	300	11 20 20·0	13 30·5
10	7 16·3	310	47·3	31·0
20	43·7	320	21 15·0	31·3
30	8 11·0	330	41·5	30·5
40	38·0	340	22 9·0	31·0
50	9 4·8	350	36·0	31·2
60	31·5	360	23 2·3	30·8
70	59·0			
80	10 26·5		Mean	13 30·9
90	53·4			= 810·90
100	11 20·3		Rate of Chron.	— ·02
			Time of 300 Vibs.	= 810·88

REDUCTION OF OBSERVATIONS MADE AT CANAAN COTTAGE.

CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829.	H. M.	°	"	°	"	"
July 8.	1 12 P. M.	+ 14·3	809·539	— 0·299	809·24	827·63
...	1 43 P. M.	+ 13·7	808·968	— 0·118	808·85	827·23
...	2 7 P. M.	+ 12·85	808·825	+ 0·135	808·96	827·34
...	3 6 P. M.	+ 10·6	808·440	+ 0·810	809·25	827·64
July 9.	11 15 A. M.	+ 16·0	810·880	— 0·99	809·89	828·29
FLAT NEEDLE.						
1829.	H. M.	°	"	°	"	
July 9.	10 18 A. M.	+ 12·75	1034·200	+ 0·21	1034·41	
...	10 47 A. M.	+ 14·0	1034·716	— 0·27	1034·446	

OF THE REDUCTION OF THE FLAT NEEDLE TO CYLINDER No. I.

1. *From the Observations of Canaan Cottage.*

1829.	H. M.			s.	
July 9.	At 10 47 A. M.	Reduced Time of 300 vibrations Flat Needle	=	1034 446	Log = 3·0147079
...	... 11 15 A. M.	Reduced Time of 300 vibrations, No. I.	=	809·89	Log = 2·9084260
		Log reduction of Flat Needle to Cylinder, No. I.		=	— 0·1062819

2. *From the Observations in Captain HALL's Garden.*

	H. M.			s.	
July 8.	At 9 31 A. M.	Reduced Time of 300 vibrations Flat Needle	=	1013·08	Log = 3·0056437
...	... 8 58 A. M.	Reduced Time of 300 vibrations, No. I.	=	798·73	Log = 2·9024000
		Log reduction of Flat Needle to No. I.		=	— 0·1032437

3. *From the Observations at Makerstoun.*

1829.	H. M.			s.	
June 29.	At 10 51 A. M.	Reduced Time of 300 vibrations Flat Needle	=	1025·23	Log = 3·0108214
...	... 9 59 A. M.	Reduced Time of 300 vibrations, No. I.	=	804·86	Log = 2·9057203
		Log reduction of Flat Needle to No. I.		=	— 0·1051011

4. *Makerstoun.*

	H. M.			s.	
June 29.	At 8 17 P. M.	Reduced Time of 300 vibrations Flat Needle	=	1024·22	Log = 3·0103933
... 28.	At 7 16 P. M.	Reduced Time of 300 vibrations, No. I.	=	803·24	Log = 2·9048453
		Log. reduction of Flat Needle to No. I.		=	— 0·1055480

As the above comparisons present considerable discordance, it will be better to defer the final reduction of the Flat Needle to that of HANSTEEN's Standard, until the result of the whole of the comparisons with No. I. have been collected.

OBSERVATIONS AT DUNDEE.

The Observations were made on that part of the Magdalene Guard called the Point.

Cylinder, No. I. First Arc 20°.

1829, July 9. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·6 / + 14·0 / Mean + 14·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 19 10·5	300	6 32 47·5	13 37·0
10	38·0	310	33 15·0	37·0
20	20 5·0	320	42·3	37·3
30	32·3	330	34 9·3	37 0
40	21 0·0	340	37·0	37·0
50	27·5	350	35 3·4	36·9
60	54·5	360	36 30·6	36·1
70	22 22·0			
80	49·0		Mean	13 36·9
90	23 16 5			= 816·90
100	43·7	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 816·88

Cylinder, No. I. First Arc 20°.

1829, July 10. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·0 / + 13·8 / Mean + 13·9
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 38 15·7	300	6 52 52·5	13 36·8
10	42·0	310	53 19·8	37·8
20	39 10·0	320	46·8	36·8
30	38·2	330	54 14·2	36·0
40	40 5·2	340	41·7	36·3
50	32·4	350	55·8	35·6
60	41 0·0	360	35·6	35·6
70	27·6			
80	55·0		Mean	13 36.413
90	42 22·3			= 816·413
100	49·6	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 816.393

Same Place.

Cylinder, No. I. First Arc 20°.

1829, July 10. P. M. Rate of Chronometer gaining 2″·5 daily.				Ther. + 13°·7 / + 13·3 / Mean + 13·5
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 57 14·0	300	7 10 50·7	13 36·7
10	41·5	310	11 17·8	36·3
20	58 8 3	320	44·7	36·4
30	36·2	330	12 12·0	35·8
40	59 3·4	340	39·0	35 6
50	30·8	350	13 6·0	35 2
60	57·8	360	33·0	35·2
70	7 0 25·8			
80	52·4		Mean	13 35·90
90	1 20·0			= 815·90
100	47·2	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 815.88

Cylinder, No. I. First Arc 20°.

1829, July 10. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 13°·3 / + 13·3 / Mean + 13·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 17 32·0	300	7 31 8·0	13 36·0
10	59·3	310	35·8	36·5
20	18 26 4	320	32 2·7	36·3
30	53·5	330	30·4	36·9
40	19 21·2	340	57·2	36·0
50	49·0	350	33 25·0	36·0
60	20 15·8	360	52·2	36·4
70	43·0			
80	21 10·3		Mean	13 36·3
90	37·6			= 816·30
100	22 4·8	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 816·28

OBSERVATIONS AT DUNDEE,—*continued.*

On the Magdalene Guard. About 300 yards to the west of the place where the preceding Observations were made.

CYLINDER, No. I. First Arc. 20°.

1829, July 10. P. M. Chronometer gaining + 2″·5 daily.				Therm. + 13°.2, + 12.8, Mean + 13.0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 29 15·8	300	8 43 52·8	13 37.0
10	42·3	310	44 20·0	37·7
20	30 10·7	320	48·0	37·3
30	38.2	330	45 15·2	37·0
40	31 5.8	340	42·4	36·6
50	33·5	350	46 10·3	36·8
60	32 0·5	360	37·0	36·5
70	28 0		Mean	13 36·986
80	55·2			= 816·986
90	33 22·8		Rate of Chron.	— 0·20
100	50·2		Time of 300 Vibs.	816·966

REDUCTION OF THE OBSERVATIONS WITH CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13.°3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's standard.
1829.	H. M.					
July 10.	6 28 P. M.	+ 14°·3	816″·88	— 0″·303	816″·580	835″·13
—	6 47 P. M.	+ 13·9	816·393	— 0·197	816·196	834·74
—	7 6 P. M.	+ 13·5	815·88	— 0·060	815·820	834·36
—	7 26 P. M.	+ 13·3	816·28	— 0·000	816·28	834·83
—	8 37 P. M.	+ 13·0	816.966	+ 0·090	817·056	835·62

OBSERVATIONS AT BRECHIN.

The Observations were made on the Inch, below the Castle.

Cylinder, No. I. First Arc 20°.

1829, July 11. A. M. Chronometer gaining + 2″·5 daily. Ther. + 16°·5, + 16·2; Mean, + 16·35.

No.	Time (h. m. s.)	No.	Time (h. m. s.)	Time of 300 Vibrations (m. s.)
0	10 2 34·5	300	10 16 12·6	13 38·1
10	3 1·7	310	40·8	39·1
20	28·5	320	17 8·0	38·5
30	56·5	330	35·5	39·0
40	4 24·0	340	18 2·4	38·4
50	51·5	350	30·0	38·5
60	5 18·3	360	56·8	38·5
70	46·0			
80	6 13·0		Mean,	13 38·586
90	40·8			= 818·586
100	7 7·5		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 818·566

Cylinder, No. I. First Arc 20°.

1829, July 11. A. M. Chronometer gaining + 2″·5 daily. Therm. + 16°·2, + 19·2; Mean, + 17·7.

No.	Time (h. m. s.)	No.	Time (h. m. s.)	Time of 300 Vibrations (m. s.)
0	10 22 43·5	300	10 36 23·0	13 39·5
10	23 11·5	310	50·5	39·0
20	38·8	320	37 18·0	39·2
30	24 6·0	330	45·0	39·0
40	33·4	340	38 12·5	39·1
50	25 0·6	350	39·6	39·0
60	28·0	360	39 6·8	38·8
70	55·2			
80	26 22·8		Mean	13 39·086
90	50·2			= 819·086
100	27 17·0		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 819·066

Same Place.

Cylinder, No. I. First Arc 20°.

1829, July 11. A. M. Chronometer gaining + 2″·5 daily. Therm. + 19°·0, + 17·8; Mean, + 18·4.

No.	Time (h. m. s.)	No.	Time (h. m. s.)	Time of 300 Vibrations (m. s.)
0	10 46 45·3	300	11 0 24·4	13 39·1
10	47 12·5	310	51·6	39·1
20	40·3	320	1 19·3	39·0
30	48 7·7	330	46·3	38·6
40	35·0	340	2 13·3	38·3
50	49 2·2	350	40·8	38·6
60	29·7	360	3 8·0	38·3
70	56·8			
80	50 24·3		Mean	13 38·714
90	51·6			= 818·714
100	51 19·0		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 818·694

OBSERVATIONS AT BRECHIN,—*continued.*

REDUCTION OF THE OBSERVATIONS, CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13.°3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's standard.
1829. July 11.	H. M. 10 11 A. M.	+ 16°35	818″566	— 0″926	817″64	836″22
—	10 31 A. M.	+ 17·7	819·066	— 1·340	817·726	836·31
—	10 55 A. M.	+ 18·4	818·694	— 1.514	817·18	835·75

OBSERVATIONS AT MONTROSE.

The Observations were made on the Links Common.

CYLINDER, No. I. First Arc 20°.

1829, July 11. P. M.

Chronometer gaining + 2″5 daily.

Ther. + 18°0, + 18·0; Mean, + 18·0

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 2 6 27·0	300	H. M. S. 2 20 3·5	M. S. 13 36·5
10	54·0	310	31·0	37·0
20	7 21·5	320	58·0	36·5
30	49·0	330	21 25·8	36·8
40	8 16·5	340	52·5	36·0
50	43·4	350	22 20·2	36·8
60	9 11·0	360	47·0	36·0
70	38·4			
80	10 5·4		Mean,	13 36·514
90	33·0			= 816·514
100	11 0·0		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 816·494

CYLINDER, No. I. First Arc 20°.

1829, July 11. P. M.

Chronometer gaining + 2″5 daily.

Ther. + 18°2, + 18·0; Mean, + 18·1

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 2 31 24·7	300	H. M. S. 2 45 1·6	M. S. 13 36·9
10	51·8	310	29·2	37·4
20	32 19·7	320	56·0	36·3
30	46·4	330	46 22·8	36·4
40	33 13·5	340	50·0	36·5
50	40·7	350	47 17·2	36·5
60	34 8·0	360	44·6	36·6
70	35·8			
80	35 2·8		Mean,	13 36·657
90	30·2			= 816 657
100	57·4		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 816·637

OBSERVATIONS AT MONTROSE,—*continued.*

The Observations were made on the Links Common.

Cylinder, No. I. First Arc 20°.

1829, July 11. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 17°·6 + 18·2 Mean, + 17·9
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 52 53·5	300	3 6 30·8	13 37·3
10	53 21·2	310	57·8	36·6
20	48·5	320	7 24·8	36·3
30	54 16·0	330	52·2	36·2
40	43·3	340	8 19·3	36·0
50	54 10·3	350	46·0	35·7
60	37·5	360	9 13·4	35·9
70	56 4·5			
80	32·3		Mean	13 36·286
90	59·8			= 816·286
100	57 26·8		Rate of Chron.	— ·02
			Time of 300 Vibs.	816·266

Cylinder, No. I. First Arc 20°.
New Station distant about ¼ of a mile.

1829, July 11. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 17°·0 + 15·8 Mean, + 16.4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	3 24 1·0	300	3 37 37·2	13 36·2
10	28·5	310	38 4·2	35·7
20	55·5	320	31·5	36·0
30	25 22·6	330	59·0	36·4
40	50·4	340	39 26·0	35·6
50	26 17·5	350	53·2	35·7
60	44·6	360	40 20·3	35·7
70	27 12·3			
80	39·7		Mean	13 35·9
90	28 6·5			= 815·90
100	33·8		Rate of Chron.	— ·02
			Time of 300 Vibs.	815·88

REDUCTION of the MONTROSE OBSERVATIONS, Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's standard.
1829. July 11.	H. M. 2 15 P. M.	+ 18°·0	816″·494	— 1″·417	815″·077	833″·53
—	2 40 P. M.	+ 18·1	816·637	— 1·420	815·217	833·74
—	3 1 P. M.	+ 17·9	816·226	— 1·414	814·812	833·33
—	3 32 P. M.	+ 16·4	815·880	— 0·939	814·94	833·46

OBSERVATIONS AT ABERDEEN.

The Observations were made in a Field about 300 yards west of Bridewell*.

Cylinder, No I. First Arc 20°.

1829, July 13. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 16°·8 + 16·0 Mean + 16·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 8 9·8	300	4 21 52·5	13 42·7
10	37·5	310	22 20·0	42·5
20	9 5·8	320	47·5	41·7
30	32·4	330	23 15·0	42·6
40	59·8	340	41·8	42·0
50	10 27·5	350	24 9·0	41·5
60	54·5	360	36·8	42·3
70	11 22·7			
80	49·8		Mean	13 42.185
90	12 17·2			= 822·185
100	45·0	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 822·165

Cylinder, No. I. First Arc 20°.

1829, July 13. P. M. Chronometer gaining + 2″·5.				Ther. + 16°·0 + 16·6 Mean + 16·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 28 31·0	300	4 42 13·0	13 42·0
10	58·2	310	40·3	42·1
20	29 26·0	320	43 7·6	41·6
30	53·2	330	35·2	42·0
40	30 21·0	340	44 3·0	42·0
50	48·5	350	30·1	41·6
60	31 16·0	360	57·6	41·6
70	43·3			
80	32 10·8		Mean	13 41.838
90	48·0			= 821.838
100	33 6·0	Rate of Chron.		— .020
		Time of 300 Vibs.		= 821.818

Same Place.

Cylinder, No. I. First Arc 20°.

1829, July 13. P. M. Chronometer gaining + 2″·5 daily.				**Ther. + 16°·0 + 16·0 Mean + 16·0**
No.	**Time.**	**No.**	**Time.**	**Time of 300 Vibrations.**
	H. M. S.		H. M. S.	M. S.
0	4 48 53·0	300	5 2 35·5	13 42·5
10	49 21·0	310	3 3·0	42·0
20	48·6	320	30·7	42·1
30	50 16·5	330	57·5	41·0
40	43·6	340	4 25·5	41·9
50	51 11·2	350	52·3	41·1
60	38·5	360	5 19·7	41·2
70	52 5·8			
80	33·0		Mean	13 41·657
90	54 1·0			= 821·657
100	28·0	Rate of Chron.		— 0·020
		Time of 300 Vibs.		= 821·637

Cylinder, No. I. First Arc 20°.

1829, July 13. P. M. Chronometer gaining + 2″·5 daily.				**Ther. + 16°·0 + 16·4 Mean + 16·2**
No.	**Time.**	**No.**	**Time.**	**Time of 300 Vibrations.**
	H. M. S.		H. M. S.	M. S.
0	5 8 52·4	300	5 22 35·0	13 42·6
10	9 21·0	310	1 23 2·6	41·6
20	48·3	320	30·8	42 5
30	10 16·0	330	57·8	41·8
40	43·3	340	24 25·3	42·0
50	11 11.0	350	52·0	41·0
60	38·0	360	25 19·2	41·2
70	12 5·3			
80	32·8		Mean	13 41·814
90	13 0·8			= 821·814
100	28·0	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 821·794

* In making these Observations I was assisted by Mr George Innes.

OBSERVATIONS AT ABERDEEN,—*continued.*

This series of Observations was made on the (Broad Hill) Links, with the assistance of Mr Ramage.

Cylinder, No. I. First Arc 20°.

1829, July 13. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 11°·5 + 11·2 Mean + 11·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 52 24·5	300	8 6 5·3	13 40·8
10	51·6	310	33·3	41·7
20	53 19·3	320	7 0·0	40·7
30	46·8	330	27·2	40·4
40	54 14·5	340	54·8	40·3
50	42·0	350	8 21·8	39·8
60	55 9·4	360	49·2	39·8
70	36·6			
80	56 3·6		Mean	13 40.50
90	31·4			= 820·50
100	58·8	Rate of Chron.		— .02
		Time of 300 Vibs.		= 820.48

REDUCTION of the ABERDEEN OBSERVATIONS with Cylinder No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13·3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	H. M.	°	″	″	″	″
July 13.	4 16 P. M.	+ 16·4	822·165	— 0·946	821·22	839·88
—	4 27 P. M.	+ 16·3	821·818	— 0·894	820·924	839·58
—	4 57 P. M.	+ 16·0	821·637	— 0·823	820·814	839·465
—	5 18 P. M.	+ 16·2	821·794	— 0·870	820·924	839·58
—	7 52 P. M.	+ 11·35	820·480	+ 0·625	821·105	839·76

OBSERVATIONS AT BANFF.

On the Battery Green.

CYLINDER, No. I. First Arc 20°.

1829, July 14. P. M. Rate of Chronometer + 2″·5 daily.				Ther. + 18°·5, + 17·7; Mean + 18·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	3 31 14·8	300	3 44 58·7	13 43·9
10	42·2	310	45 26·8	44·6
20	32 10·0	320	53·5	43·5
30	47·8	330	46 21·3	43·5
40	33 4·8	340	48·5	43·7
50	33·0	350	47 16·2	43·2
60	34 0·0	360	43·5	43·5
70	28·0			
80	55·3		Mean	13 43·70
90	35 22·8			= 823·70
100	50·0		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 823·68

In a Field west of Battery Green.

CYLINDER, No. I. First Arc 20°.

1829, July 14. P. M. Rate of Chronometer + 2″·5 daily.				Ther. + 17°·3, + 18·0; Mean + 17·65
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 7 27·5	300	4 21 11·7	13 44·2
10	55·0	310	39·8	44·8
20	8 22·8	320	22 6·7	43·9
30	50·0	330	34·5	44·5
40	9 17·8	340	23 2·0	44·2
50	45·2	350	29·5	44·3
60	10 12·8	360	56·8	44·0
70	40·0			
80	11 7·5		Mean	13 44·27
90	35·8			= 824·27
100	12 2·4		Rate of Chron.	— ·02
			Time of 300 Vibs.	= 824·25

In a Field at a considerable distance to the south-west of the Battery Green.

CYLINDER, No. I. First Arc 20°.

1829, July 14. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 19°·0, + 18·0; Mean + 18·5
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 43 57·5	300	4 57 43·0	13 45·5
10	44 26·2	310	58 10·8	44·6
20	53·2	320	37·8	44·6
30	45 21·2	330	59 5·5	44·3
40	48·5	340	33·0	44·5
50	46 16·6	350	5 0 0·2	43·6
60	43·5	360	28·0	44·5
70	47 11·0			
80	39·0		Mean	13 44·514
90	48 6·3			= 824·514
100	34·0		Rate of Chron.	— ·023
			Time of 300 Vibs.	= 824·490

CYLINDER, No. I. First Arc 20°.

1829, July 14. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 18°·0, + 16·0; Mean + 17·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	5 5 0·6	300	5 18 45·2	13 44·6
10	28·3	310	19 12·8	44·5
20	55·8	320	40·0	44·2
30	6 23·5	330	20 7·5	44·0
40	51·2	340	35·0	43·8
50	7 19·0	350	21 2·4	43·4
60	46·2	360	29·8	43·6
70	8 13·0			
80	41·0		Mean	13 44·014
90	9 8·5			= 824·014
100	36·5		Rate of Chron.	— ·023
			Time of 300 Vibs.	= 823·99

REDUCTION of the OBSERVATIONS made at BANFF with Cylinder No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	H. M.	°	″	″	″	″
July 14.	3 39 P. M.	+ 18·1	823·68	— 1·467	822·213	841·09
—	4 16 P. M.	+ 17.65	824·25	— 1·330	822·92	841·62
—	4 52 P. M.	+ 18·5	824·49	— 1·590	822·90	841·60
—	5 13 P. M.	+ 17·0	823·99	— 1·131	822·86	841·56

OBSERVATIONS AT PORTSOY.

At the south-east end of the Town, near the Toll-Bar.

Cylinder, No. I. First Arc 20°. Cylinder, No. I.

1829, July 15. A. M. Rate of the Chronometer + 2″·5				Ther. + 19°·2, + 19·0, Mean + 19·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	9 23 5·5	300	9 36 46·2	13 40·7
10	33·7	310	37 14·0	40·3
20	24 1·2	320	41·0	39·8
30	28·5	330	38 8·5	40·0
40	55·7	340	35·6	39·9
50	25 23·2	350	39 3·0	39·8
60	50·5	360	30·6	40·1
70	26 17·8			
80	45·2		Mean	13 40·086
90	27 12·4			= 820·086
100	39·8	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 820·063

1829, July 14. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 19°·0, + 18·3, Mean + 18·65
No.	Time.	No.	Time	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	9 42 21·0	300	9 56 1·5	13 40·5
10	48·5	310	29·3	40·8
20	43 16·0	320	55·8	39·8
30	43·2	330	57 23·4	40·2
40	44 10·2	340	49·5	39·3
50	37·6	850	58 16·0	38·4
60	45 5·0	360	43·4	38·4
70	32·8			
80	59·8		Mean	13 39·63
90	46 27·5			= 819·63
100	54.8	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 819·61

REDUCTION of the PORTSOY OBSERVATIONS, with Cylinder No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829,	H. M.	°	″	″	″	″
July 14.	9 31 A. M.	+ 19·1	820·063	— 1·763	818·3	836·56
—	9 50 A. M.	+ 18·65	819·61	— 1·638	817·972	836·9

OBSERVATIONS AT ELGIN.

The Observations were made on the Gallows Green.

CYLINDER, No. I. First Arc 20°. CYLINDER, No. I.

1829, July 16. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·7, + 15·6, Mean + 15·15
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 26 25·8	300	8 40 13·5	13 47·7
10	53·2	310	41·8	48·6
20	27 21·0	320	41 9·2	48·2
30	48·7	330	36·0	48·3
40	28 16·3	340	42 3·5	47·2
50	44·0	350	31·6	47·6
60	29 11·7	360	59·0	47·3
70	39·8			
80	30 6·8		Mean	13 47·842
90	35·0			= 827·842
100	31 2·0	Rate of Chron.		— ·022
		Time of 300 Vibs.		= 827·82

1829, July 16. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 15°·6, + 17·0, Mean + 16·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 47 56·0	300	9 1 44·5	13 48·5
10	48 24·0	310	2 12·6	47·6
20	51·8	320	40·0	48·2
30	49 19·8	330	3 7·3	47·5
40	47·6	340	35·3	47·7
50	50 15·0	350	4 2·6	47·6
60	42·5	360	30·0	47·5
70	51 10·2			
80	48·2		Mean	13 47·80
90	52 5·2			= 827·80
100	33·0	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 827.777

REDUCTION OF THE ELGIN OBSERVATIONS, WITH CYLINDER No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829. July 16.	H. M. 8 35 A. M.	+ 15°·5	827″·82	— 0″·675	827″·145	845″·94
—	8 56 A. M.	+ 16·3	827·777	— 0·921	826·856	845·65

OBSERVATIONS AT FORRES.

The Observations were made in a Field near the East end of the Town.

Cylinder, No. I. First Arc 20°. Cylinder, No. I.

1829, July 16. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 18°·2 + 18·0 Mean + 18·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 30 37·0	300	2 44 24·0	13 47·0
10	31 5·0	310	51·0	46·0
20	33·0	320	45 19·0	46·0
30	32 0·2	330	46·2	46·0
40	28·3	340	46 13·2	45·9
50	55·8	350	42·0	46·2
60	33 23·2	360	47 10·0	46·8
70	50·8			
80	34 18·5		Mean	13 46·271
90	45·8			= 826·271
100	35 13·0		Rate of Chron.	— ·023
			Time of 300 Vibs.	= 826·248

1829, July 16. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 18°·0 + 18·4 Mean + 18·2
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 50 45·8	300	3 4 33·0	13 47·2
10	51 14·0	310	5 0·8	46·8
20	42·0	320	28·3	46·3
30	52 9·3	330	55·2	45·9
40	37·2	340	6 23·4	46·2
50	53 4·7	350	51·2	46·5
60	32·3	360	7 18·8	46·5
70	59·7			
80	53 27·6		Mean	13 46·486
90	54·8			= 826·486
100	55 22·0		Rate of Chron.	— ·023
			Time of 300 Vibs.	— 826·463

REDUCTION of the FORRES OBSERVATIONS with Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 16.	H. M. 2 59 P. M.	+ 18°.1	826″·248	— 1″·471	824″·777	843″·52
—	2 59 P. M.	+ 18.2	826·463	— 1·503	824·960	843·71

OBSERVATIONS AT NAIRN.

The Observations were made in the Field adjoining to the Bridge at the south-east end of the Town.

CYLINDER, No. I. First Arc. 20°.

1829, July 16. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·0 + 12·7 Mean + 13·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 58 14·5	300	8 11 57·5	13 43·0
10	42·0	310	12 25·0	43·0
20	59 9·8	320	52·0	43·2
30	37·2	330	13 20·2	43·0
40	8 0 4·7	340	47·8	42·8
50	31·8	350	14 15·0	43·2
60	59·8	360	42·3	42·5
70	1 27·2			
80	54·8		Mean	13 42·956
90	2 22·2			= 822·956
100	49·5	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 822.933

CYLINDER, No. I. First Arc 20°.

1829, July 16. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 12°· + 11·6 Mean + 12.15
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 16 21·2	300	8 30 4·0	13 43·8
10	48·8	310	32·0	43·2
20	17 16·8	320	59·3	42·5
30	44·3	330	31 27·0	42·7
40	18 11·6	340	54·7	43·1
50	39·2	350	22·0	42·8
60	19 6·2	360	49·0	42·8
70	34·0			
80	20 1·3		Mean	13 42·986
90	29·0			= 822·986
100	56·3	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 822·963

CYLINDER, No. I. First Arc 30°.

1829, July 16. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 11°·6 + 11·0 Mean + 11·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 35 36·8	300	8 49 20·8	13 44·0
10	36 4·5	310	48·0	43·5
20	32·3	320	50 15·4	43·1
30	37 0·0	330	42·8	42·8
40	27·5	340	51 11·0	43·5
50	55·0	350	38·0	43·0
60	38 22·4	360	52 5·6	43·2
70	50·2			
80	39 17·2		Mean	13 43·3
90	45·0			= 823·300
100	40 12·3	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 823·277

REDUCTION of the OBSERVATIONS at NAIRN, with Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
	H. M.	°	"	"	"	"	"
1829. July 16.	8 6 P. M.	+ 13·35	822·933	— 0·013		822·92	841·62
—	8 25 P. M.	+ 12·15	822·963	— 0·351		822·612	841·30
—	8 44 P. M.	+ 11·3	823·277	+ 0·611	— 0·784	823·104	841·81

OBSERVATIONS AT FORT GEORGE.

The Observations were made on the Level Plane, at the distance of about 2 Miles to the south-east of the Fort.

Cylinder, No. I. First Arc 20°.

1829, July 17. Noon. Rate of Chronometer + 2·5 daily. Ther. + 18·0 / + 17·2 / Mean + 17·6

No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	11 59 27·8	300	12 13 16·5	13 48·7
10	55·4	310	43·8	48·4
20	12 0 23·0	320	14 11·0	48·0
30	51·0	330	39·0	48·0
40	1 19·0	340	15 6·5	47·5
50	46·2	350	33·8	47·6
60	2 14·2	360	16 1·2	47·0
70	41·6			
80	3 9·2		Mean	13 47·889
90	37·0			= 827·889
100	4 4·3	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 827·866

Cylinder, No. I. First Arc 20°.

1829, July 17. P. M. Rate of Chronometer + 2·5 daily. Ther. + 17·2 / + 16·0 / Mean + 16·6

No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	12 20 48·8	300	12 34 36·8	13 48·0
10	21 17·5	310	35 5·0	47·5
20	44·0	320	32·5	48·5
30	22 12·0	330	59.7	47·7
40	39·7	340	36 27·4	47·7
50	23 7·5	350	55·0	47·5
60	35·0	360	37 22·3	47·3
70	24 3·3			
80	39·9		Mean	13 47·743
90	57·5			= 827·743
100	25 25·5	Rate of Chron.		— ·023
		Time of 300 Vibs.		= 827·720

OBSERVATIONS AT FORT GEORGE,—*continued.*

These Observations were made in the same place with those of Cylinder No. I.

Flat Needle. First Arc 20°.

1826, July 17. P. M. Rate of Chronometer, + 2″5 daily.				Ther. + 17°0, 17·2; Mean, + 17·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	12 42 4·8	300	12 59 41·2	17 36·4
10	40·0	310	1 0 16·5	36·5
20	43 16·0	320	51·7	35·7
30	51·2	330	1 26·8	35·6
40	44 27·0	340	2 2·2	35·0
50	45 2·0	350	37·2	35·2
60	37·5	360	3 12·5	35·0
70	46 12·5	370	47·5	35·0
80	47·8	380	4 23·0	35·2
90	47 23·0	390	57·8	34·8
100	58·2	400	5 33·0	34·8
			Mean,	17 35.383
			=	1055·383
Rate of Chronometer,			—	·030
Time of 300 Vibrations,			=	1055·353

Flat Needle. First Arc 20°.

1826, July 17. P. M. Rate of Chronometer + 2″.5 daily.				Ther. + 17°·2, + 17·6; Mean, + 17·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	1 10 14·0	300	1 27 50·0	17 36·0
10	49·2	310	28 25·4	36·2
20	11 25·2	320	29 0·7	35·5
30	12 0·4	330	36·0	35·6
40	35·5	340	30 11·0	35·5
50	13 10·8	350	46·0	35·2
60	46·0	360	31 21·2	35·2
70	14 21·3	370	56·6	35.3
80	56·5	380	32 31·6	35·1
90	15 32·0	390	33 6·8	34·8
100	16 7·3	400	42·0	34.7
			Mean,	17 35·373
			=	1055·373
Rate of Chronometer,			—	·030
Time of 300 Vibrations,			=	1055·343

REDUCTION OF THE OBSERVATIONS MADE AT FORT GEORGE.

Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 17.	H. M. 12 8 P. M.	+ 17°6	827″866	— 1″321	826″545	845″33
...	12 29 P. M.	+ 16·6	827·720	— 1·014	826·706	845·49
Flat Needle.						
July 17.	12 54 P. M.	+ 17·1	1055·353	— 1·488	1053·865	
...	1 23 P. M.	+ 17·4	1055·343	— 1·605	1053·738	

July 17. At 1 H. 8 M. P. M. red. Time of 300 vibrations with flat needle = 1053·8 S. Log. = 3·0227582
... 12 18 P. M. red. Time of 300 vibrations, cylinder, No. I. = 826·625 Log. 2·9173086

Log. reduction of the flat needle to Cylinder, No. 1. = — 0·1054496

OBSERVATIONS AT TAIN.

The Observations were made in a Field near the south-east end of the Town.

Cylinder, No. I. First Arc 20°.

1829, July 18. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·4 15·0 Mean, + 14·7
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 3 54·8	300	2 17 49·4	13 54·2
10	4 23·0	310	18 16·5	53·5
20	50·5	320	43·7	53·2
30	5 18·8	330	19 11·6	52·8
40	46·5	340	38·8	52·3
50	6 14·3	350	20 6·3	52·0
60	42·0	360	33·5	51·5
70	7 9·7			
80	37·2		Mean,	13 52·786
90	8 5·3			= 832·786
100	33·0		Rate of Chron. —	·026
			Time of 300 Vibs.	832·760

Cylinder, No. I. First Arc 20°.

1829, July 18. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 15°·0 + 18·0 Mean, + 16·5
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 23 33·4	300	2 37 27·5	13 54·1
10	24 0·6	310	54·6	54·0
20	28·2	320	38 22·0	53·8
30	56·0	330	49·6	53·6
40	25 24·0	340	39 17·3	53·3
50	51·8	350	44·4	52·6
60	26 19·8	360	40 11·8	52·0
70	52·5			
80	27 15·2		Mean,	13 53·074
90	43·0			= 833·074
100	28 11·0		Rate of Chron. —	·024
			Time of 300 Vibs.	833·05

Same Place with those of No. I. Cylinder.

Flat Needle. First Arc 20°.

1829, July 18. P. M. Chronometer gaining + 2″·5 daily.				Ther + 17°·6 + 14·5 Mean, + 16·05
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	2 48 56·5	300	3 6 41·0	17 44·5
10	49 32·0	310	7 16·4	44·4
20	50 7·8	320	51·8	44·0
30	43·8	330	8 28·0	44·2
40	51 19·7	340	9 3·5	43·8
50	55·0	350	38·8	43·8
60	52 31·0	360	10 14·5	43·5
70	53 5·8	370	49·6	43·9
80	41·5	380	11 24·8	43·3
90	54 16·5	390	12 0·0	43·5
100	52·3	400	35·0	42·7
			Mean,	17 43·78
			=	1063·78
	Rate of Chronometer,		—	·03
	Time of 300 Vibrations,			1063·75

Flat Needle. First Arc 20°.

1829, July 18. P. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·5 + 14·2 Mean, = 14·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	3 16 8·3	300	3 33 52·7	17 44·4
10	44·8	310	34 28·0	44·2
20	17 20·4	320	35 3·4	44·0
30	56·0	330	39·0	44·0
40	18 31·8	340	36 14·6	43·8
50	19 7·0	350	49·8	42·8
60	42·4	360	37 25·2	42·8
70	20 18·5	370	38 0·7	42·2
80	53·8	380	36·0	42·2
90	21 29·5	390	39 11·5	42·0
100	22 5·0	400	47·0	42·0
			Mean,	17 43·09
			=	1063·09
	Rate of Chronometer,		—	·03
	Time of 300 Vibrations,		=	1063·06

REDUCTION OF THE TAIN OBSERVATIONS, WITH CYLINDER, No. I.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829. July 18.	h. m. 2 12 P. M.	+ 14·7	832·760	— 0·428	832·33	851·24
...	2 32 P. M.	+ 16·5	833·050	— 0·989	832·06	850·97
July 18.	h. m. 3 2 P. M.	+ 16·05	1063·75	— 0·99	1062·76	
...	3 28 P. M.	+ 14·35	1063·06	— 0.329	1062·73	

July 18. At 3 15 P. M. Reduced time of 300 vibrations Flat Needle = 1062·745 Log = 3·0264292
At 2 22 P. M. Reduced time of 300 vibrations No. I. = 832·195 Log = 2·9202252

Log Reduction of the Flat Needle to No. I. = — 0·1062040

OBSERVATIONS AT DORNOCH.

The Observations were made in the Garden at the Inn.

FLAT NEEDLE. First Arc 40°.

1829, July 19. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 12°0 + 12·5 Mean, + 12·25
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	h. m. s. 7 28 12·5	300	h. m. s. 7 46 0·8	m. s. 17 48·3
10	48·5	310	37·0	48·5
20	29 24·8	320	47 12·2	47·4
30	30 0·5	330	47·2	46·7
40	36·5	340	48 23·0	46·5
50	31 12·4	350	58·3	46·1
60	48·5	360	49 33·5	45·0
70	32 24·5	370	50 9·0	45·5
80	59·5	380	44·5	45·0
90	33 35·8	390	51 20·0	44·2
100	34 11·2	400	55·8	44·6
			Mean, .	17 46·16
			=	1066·16
	Rate of Chronometer,		—	·03
	Time of 300 Vibrations,		=	1066·13

FLAT NEEDLE. First Arc 40°.

1829, July 19. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 12°5 + 13·3 Mean, + 12·9
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	h. m. s. 7 55 8·2	300	h. m. s. 8 12 56.8	m. s. 17 48·6
10	44·0	310	13 31·8	47·8
20	56 20·5	320	14 7·3	46·8
30	56·0	330	42·3	46·2
40	57 32·0	340	15 18·0	46·0
50	58 7·6	350	53.3	45·7
60	43·5	360	16 29·0	45·5
70	59 19·0	370	17 4·0	45·0
80	55·0	380	40·2	45·2
90	8 0 30·8	390	18 16·0	45·2
100	1 6·2	400	51·5	45·3
			Mean, .	17 46·12
			=	1066·12
	Rate of Chronometer,		—	·03
	Time of 300 Vibrations,		=	1066·09

OBSERVATIONS AT DORNOCH,—*continued.*

The Observations were made in the Garden at the Inn.

Cylinder, No. I. First Arc 40°.

1829, July 19. A. M. Chronometer gaining + 2″.5 daily.				Ther. + 13°.3 + 13.3 Mean + 13·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 29 55·0	300	8 43 52·0	13 57·0
10	30 24·0	310	44 20·2	56·2
20	52·0	320	47·8	55·8
30	31 21·0	330	45 15·8	54·8
40	49·0	340	43·4	54·4
50	32 17·0	350	46 11·0	54·0
60	45·2	360	35·0	53·3
70	33 12·8	370	47 5·5	52·7
80	41·0	380	33·3	52·3
90	34 9·0	390	48 1·3	52·3
100	36·8	400	29·0	52·3
			Mean,	13 54·09
				= 834·09
	Rate of Chronometer,			— ·02
	Time of 300 Vibrations,			= 834·07

Cylinder, No. I. First Arc 40°.

1829, July 19. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 13°·3 13·2 Mean, + 13·25
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 50 16·8	300	9 4 12·8	13 56·0
10	45·2	310	40·7	55·5
20	51 13·2	320	5 8·5	55·3
30	42·0	330	36·0	54·0
40	52 10·0	340	6 3·8	53·8
50	38·0	350	32·0	54·0
60	53 6·0	360	59·2	53.2
70	34·5	370	7 27·0	52·5
80	54 2·0	380	55·0	53·0
90	30·0	390	8 22·8	52·8
100	57·6	400	50·5	52·9
			Mean,	13 53·91
				= 833.91
	Rate of Chronometer,			— ·02
	Time of 300 Vibrations,			= 833.89

REDUCTION of the DORNOCH OBSERVATIONS, with Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Therm.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibs. reduced to Hansteen's Standard.
1829. July 19.	H. M. 8 39 A. M.	+ 13°·3	834″·07	0″·000	— 0″·967	833″·103	852″·03
...	8 59 A. M.	+ 13·25	833.89	+ 0·015	— 0·967	832·938	851·86
			Flat Needle.				
July 19.	7 40 A. M.	+ 12·25	1066·13	+ 0·415	— 1·235	1065·31	
...	8 7 A. M.	+ 12· 9	1066·09	+ 0·157	— 1·235	1064·70	

July 19. At 7h 53′ A. M. Red. time of 300 vibs. Flat Needle = 1065″·00 = log. 3·0273496
... At 8 49 A. M. Red. time of 300 vibs. No. I. = 833·02 = log. 2·9206554

Log. Reduction of the Flat Needle to Cylinder, No. I. — 0·1066942

OBSERVATIONS AT HELMSDALE.

The Observations were made in the Field in which the Old Castle stands; about 150 yards west of the Ruins.

Cylinder, No. I. First Arc 40°.

1829, July 19. P. M. Chronometer gaining + 2″·5.				Ther. + 17°·2 + 17·0 Mean, + 17·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 49 15·7	300	5 3 14·0	13 58·3
10	44·2	310	42·0	57·8
20	50 12·7	320	4 9·5	57·1
30	41·3	330	37·6	57·3
40	51 9·5	340	5 5·5	56·0
50	38·0	350	33·2	55·2
60	52 5·5	360	6 1·0	55·5
70	34·2	370	28·8	54·6
80	53 2·5	380	56·5	54·0
90	31·0	390	7 24·0	53·0
100	58·8	400	51·8	53·0
			Mean,	13 55.62
				= 835·62
Rate of Chronometer,				— ·02
Time of 300 Vibrations,				= 835·60

Cylinder, No. I. First Arc 40°.

1829, July 19. P. M. Chronometer gaining + 2″·5.				Ther. + 17°·0 + 16·7 Mean, + 16·85
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	5 11 8·6	300	5 25 7.0	13 58·5
10	37·5	310	35·2	57·7
20	12 5·5	320	26 2·6	57·1
30	34·4	330	30·6	56·2
40	13 2·4	340	58·5	56·1
50	31·2	350	27 26·3	55·1
60	58·6	360	53·0	54·4
70	14 27·0	370	28 21·7	54·7
80	55·5	380	49·5	54·0
90	15 24·0	390	29 17·0	53·0
100	51·7	400	44·8	53·2
			Mean,	13 55·445
				= 835·445
Rate of Chronometer,				— ·025
Time of 300 Vibrations,				= 835·42

REDUCTION of the HELMSDALE OBSERVATIONS, with Cylinder, No. I.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 19.	H. M. 4 57 P. M.	17°·1	835″·60	— 1″·178	— 0″·97	833″·43	852″·37
...	5 19 P. M.	16·85	835·42	— 1·100	— 0·97	833·35	852·28

OBSERVATIONS AT WICK

The Observations were made in a Field adjoining the west end of the Town, on the road to Thurso.

Cylinder, No. I. First Arc 40°.

1829, July 20. P. M. Chronometer gaining + 2″.5.				Ther. + 14°.0 + 13.2 Mean, + 13.6
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	5 25 13·0	300	5 39 12·5	13 59·5
10	42·3	310	41·0	58·7
20	26 11·5	320	40 9·0	57·5
30	40·0	330	38·8	58·8
40	27 8·2	340	41 4·8	56·6
50	37·0	350	32·6	55·6
60	28 5·0	360	42 0·7	55·7
70	33·0	370	28·5	55·5
80	29 1·0	380	56·3	55·3
90	29·6	390	43 24·2	54·6
100	57·5	400	52·2	54·7
			Mean,	13 56·59
			=	836·59
Rate of Chronometer,			—	·02
Time of 300 Vibrations,			=	836·57

Cylinder, No. I. First Arc 40°.

1829, July 20. P. M. Chronometer gaining + 2″.5 daily.				Ther. + 13°.2 + 13.0 Mean, + 13.1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	5 48 27·4	300	6 2 27·0	13 59·6
10	56·7	310	55·0	59·3
20	49 26·0	320	3 23·2	57·2
30	54·6	330	51·6	57·0
40	50 22·8	340	4 19·0	57·2
50	51·5	350	47·0	55·5
60	51 19·3	360	5 14·7	55·4
70	47·5	370	42·5	55·0
80	52 15·6	380	6 10·4	54·8
90	44·0	390	38·2	54·2
100	53 12·0	400	7 6·0	54·0
			Mean,	13 56·29
			=	836·29
Rate of Chronometer,			—	·02
Time of 300 Vibrations,			=	836·27

REDUCTION OF THE WICK OBSERVATIONS.

Cylinder, No. I. First Arc. 40°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc. 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 20.	H. M. 5 33 P. M.	+ 13°·6	836″·57	— 0″·090	— 0″·97	835″·51	854″·49
...	5 57 P. M.	+ 13·1	836·27	— 0·060	— 0.97	835·24	854·22

OBSERVATIONS AT THURSO.

The Observations were made west of the Town, on the Pennyland Estate, in the field in front of the House.

Cylinder, No. I. First Arc 40°.

1829, July 21. A. M. Rate of Chron. + 2″·5.				Ther. + 14°·5 + 14·0 Mean + 14·25
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	10 30 1·5	300	10 44 0·0	13 58·5
10	30·2	310	28·2	58·0
20	58·0	320	55·8	57·8
30	31 27·0	330	45 24·8	57·8
40	54·0	340	52·2	58·0
50	32 23·3	350	46 20·0	56·7
60	50·5	360	48·0	57·5
70	33 19·0	370	47 15·2	56·2
80	47·0	380	43·0	56·0
90	15·0	390	48 10·8	55·8
100	43·0	400	38·0	55·0
			Mean	13 57·27
				= 837·27
Rate of Chronometer				— ·02
Time of 300 Vibrations				= 837·25

Cylinder, No. I. First Arc 40°.

1829, July 21. A. M. Rate of Chron. + 2″·5.				Ther. + 14°·0 + 15·2 Mean + 14·6
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	10 52 38·0	300	11 6 37·0	13 59·0
10	53 6·8	310	7 5·0	58·2
20	35·5	320	33·0	57·5
30	54 3·2	330	8 0·6	57·4
40	31·3	340	28·0	57·7
50	59·5	350	56·0	56·5
60	55 27·8	360	9 23·8	56·0
70	55·5	370	51·7	56·2
80	56 23·3	380	10 19·7	56·4
90	51·3	390	47·3	56·0
100	57 19·4	400	11 15·0	55·6
			Mean	13 56.954
				= 836.954
Rate of Chronometer				— ·024
Time of 300 Vibrations				= 836·93

Cylinder, No. I. First Arc 40°.

1829, July 21. A. M. Rate of Chron. + 2″·5.				Ther. + 15°·2 + 17·0 Mean + 16·1
No	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	11 14 38·0	300	11 28 37·0	13 59·0
10	15 7·0	310	29 5·8	58·8
20	35·8	320	34·2	58·4
30	16 4·0	330	30 2·0	58·0
40	32·5	340	29·6	57·1
50	17 0·3	350	57·4	57·2
60	28·0	360	31 25·0	57·0
70	56·0	370	52.7	56·7
80	18 34·5	380	32 21·0	56·5
90	52·0	390	48·6	56·6
100	19 20·0	400	33 16·4	56·4
			Mean	13 57·427
				= 837·427
Rate of Chronometer				— ·027
Time of 300 Vibrations				= 837·400

Flat Needle. First Arc 30°.

1829, July 21. A. M. Rate of Chron. + 2″·5.				Ther. + 17°·0 + 15·8 Mean + 16·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	11 44 34·5	300	12 2 25·2	17 50·7
10	45 10·2	310	3 0·5	50·3
20	46·8	320	36·0	49·2
30	46 22·8	330	4 11·2	48·4
40	58·0	340	45·8	47·8
50	47 34·6	350	5 21·2	46·6
60	48 9·8	360	56·0	46·2
70	45·8	370	6 31·0	45·2
80	49 21·3	380	7 6·3	45·0
90	56·6	390	41·4	44·8
100	50 32·0	400	8 16·6	44·6
			Mean	17 47·163
				= 1067·163
Rate of Chronometer				— ·033
Time of 300 Vibrations				= 1067·13

REDUCTION of the THURSO OBSERVATIONS.

Cylinder, No. I. First Arc 40°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	H. M.	°	"	"	"	"	"
July 21.	10 38 A. M.	+ 14·25	837·25	— 0·290	— 0·97	835·99	854·99
—	11 1 A. M.	+ 14·6	836·93	— 0·400	— 0·97	835·56	854·55
—	11 23 A. M.	+ 16·1	837·40	— 0·87	— 0·97	835·56	854·55
			Flat Needle. First Arc 30°.				
—	H. M. 11 57 A. M.	° + 16·4	" 1067·13	" — 1·23	" — 1·24	" 1064·66	

Log Reduction of the Flat Needle to No. I. = — 0·1051618.

OBSERVATIONS AT TONGUE.

The Observations were made in the Garden, in front of the Inn.

Cylinder, No. I. First Arc 40°.

1829, July 22. P. M. Chronometer gaining + 2"·5.				Ther. + 13°·5 + 12·7 Mean + 13·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 32 31·0	300	6 46 29·7	13 58·7
10	59·5	310	57·4	57·9
20	33 28·7	320	47 26·0	57·3
30	56·5	330	53·4	56·9
40	34 25·3	340	48 21·0	55·7
50	53·0	350	49·2	55·2
60	35 21·3	360	49 16·2	54·9
70	49·0	370	44·0	55·0
80	36 17·5	380	50 11·7	54·2
90	45·5	390	39·6	54·1
100	37 13·2	400	51 7·4	54·2
			Mean	13 55.83
			=	835·83
	Rate of Chronometer		—	·02
	Time of 300 Vibrations		=	835·81

Cylinder, No. I. First Arc 40°.

1829, July 22. P. M. Chronometer gaining + 2"·5.				Ther. + 12°·7 + 12·4 Mean + 12·55
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 53 26·7	300	7 7 24·0	13 57·3
10	54·8	310	51·5	56·7
20	54 23·2	320	8 19·0	55·8
30	51·3	330	47·3	56·0
40	55 19·8	340	9 15·0	55·2
50	47·5	350	43·0	55·5
60	56 15·8	360	10 11·0	55·2
70	44·0	370	38·6	54·8
80	57 12·0	380	11 6·6	54·6
90	40·0	390	34·4	54·4
100	58 7·7	400	12 2·0	54·3
			Mean	13 55·44
			=	835·44
	Rate of Chronometer		—	.02
	Time of 300 Vibrations		=	835·42

OBSERVATIONS AT TONGUE,—*continued.*

Cylinder, No. 1. First Arc 40°.

1829, July 22. P. M. Chronometer gaining + 2″·5

Ther. + 12°·4, + 12·2; Mean + 12·3

No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	7 15 45·5	300	7 29 43·0	13 57·5
10	16 13·8	310	30 11·0	57·2
20	42·0	320	39·0	57.0
30	17 10·8	330	31 7·0	56.2
40	38·5	340	35·0	56·5
50	18 7·0	350	32 3·0	56·0
60	35·5	360	30·5	55·0
70	19 3·0	370	57·5	54·5
80	31·2	380	33 25·3	54·1
90	59·0	390	53·0	54·0
100	20 27·0	400	34 20·8	53·8

Mean 13 55·62
= 835·62
Rate of Chronometer — ·02
Time of 300 Vibrations = 835·60

Flat Needle. First Arc 30°.

1829, July 22. P. M. Chronometer gaining + 2″·5

Ther. + 12°·2, + 11·5; Mean + 11·85

No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	7 39 43·0	300	7 57 33·0	17 50·0
10	40 19·0	310	58 7·8	48·8
20	55·0	320	43·0	48·0
30	41 31·7	330	59 18·8	47·1
40	42 7·0	340	54·2	47·2
50	43·0	350	8 0 30·0	47·0
60	43 19·0	360	1 5·0	46·0
70	54·0	370	41·8	47·8
80	44 31·2	380	2 16·5	45·3
90	45 6·0	390	52·0	46·0
100	41·8	400	3 27·5	45·7

Mean 17 47·17
= 1067·17
Rate of Chronometer — ·03
Time of 300 Vibrations = 1067·14

Cylinder, No. 1. First Arc 40°.

1829, July 23. A. M. Chronometer gaining + 2″·5.

Ther. + 11°·3, + 12·0; Mean + 11·65

No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	9 5 43·5	300	9 19 42·3	13 58·8
10	6 12·6	310	20 11·2	58·6
20	41·4	320	39·5	58·1
30	7 9·5	330	21 7·5	58·0
40	38·2	340	35·0	56·8
50	8 5·8	350	22 3·0	57·2
60	33·4	360	30·6	57·2
70	9 1·5	370	58·0	56·5
80	30·0	380	23 26·3	56·3
90	57·7	390	54·0	56·3
100	10 25·5	400	24 22·0	56·5

Mean 13 57·3
= 837·3
Rate of Chronometer — 0·2
Time of 300 Vibrations = 837·28

Cylinder, No. 1. First Arc 40°.

1829, July 23. A. M. Chronometer gaining + 2″5.

Ther. + 12°·0, + 13·2; Mean + 12·6

No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	9 30 4·5	300	9 44 3·5	13 59·0
10	33·3	310	32·0	58·7
20	31 1·7	320	45 0·0	58·3
30	30·8	330	28·2	57·4
40	58·7	340	56·4	57·7
50	32 27·0	350	45 24·4	57·4
60	55·0	360	52·0	57·0
70	33 23·6	370	46 20·0	56·4
80	51·5	380	47·8	56·3
90	34 19·4	390	47 15·8	56·4
100	47·2	400	43·7	56·5

Mean 13 57·373
= 837·373
Rate of Chronometer — ·023
Time of 300 Vibrations = 837·35

REDUCTION of the OBSERVATIONS at TONGUE.

Cylinder, No. I. First Arc 40°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibs.	Reduction to + 13°·3.	Reduction to Arc 40°.	Red. Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 22.	H. M. 6 41 P. M.	° + 13·1	″ 835·81	″ + 0·062	″ — 0·972	″ 834·90	″ 853·87
—	7 2 P. M.	+ 12·55	835·42	+ 0·232	— 0·972	834·78	853·75
—	7 24 P. M.	+ 12·3	835·60	+ 0·310	— 0·972	834·94	853·91
July 23.	9 14 A. M.	+ 11·65	837·28	+ 0·513	— 0·973	836·82	855·83
—	9 38 A. M.	+ 12·6	837·35	+ 0·217	— 0·973	836·594	855·60
Flat Needle. First Arc. 30°.							
July 22.	7 51 P. M.	+ 11·85	1067·14	+ 0·574	— 0·682	1067·03	

July 22. at 7 H. 2 P. M. No. 1. = 853·843 s.
... 23. at 9 26 A. M. No. 1. = 855·715
Mean = 854·78 s.

Daily Variation = 1·862

Log. Red. of Flat Needle to No. 1. = — 0·1065578.

OBSERVATIONS AT INVER-BAGASTY (West end of Loch Naver.)

The Observations were made in the Field in front of the Inn.

Cylinder, No. I. First Arc 40°.

1829, July 23. P. M. Chronometer gaining + 2″·5. Ther. + 10°·3, + 9·4, Mean + 9·85

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 8 1 37·8	300	H. M. S. 8 15 33·2	M. S. 13 55·4
10	2 6·0	310	16 1·2	55·2
20	35·0	320	29·0	54·0
30	3 2·5	330	56·5	54·0
40	31·2	340	17 24·0	53·8
50	59·0	350	51·5	52·5
60	4 27·0	360	18 19·6	52 6
70	54·5	370	47·0	52·5
80	5 23·0	380	19 14·5	51·5
90	51·0	390	42·0	51·0
100	6 18·8	400	20 9·8	51·0
			Mean	13 53·04
				= 833·04
			Rate of Chronometer	— ·02
			Time of 300 Vibrations	= 833·02

Cylinder, No. I. First Arc 40°.

1829, July 23. P. M. Chronometer gaining + 2″·5 daily. Ther. + 9°·4, + 8·2, Mean + 8·8

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 8 22 28·0	300	H. M. S. 8 36 23·2	M. S. 13 55·2
10	56·4	310	51·0	54·6
20	23 25·2	320	37 19·2	54·0
30	53·0	330	46·5	53·5
40	24 21·3	340	38 14·2	52·9
50	49·3	350	42 0	52·7
60	25 18·0	360	39 9·8	51·8
70	45·4	370	37·3	51·9
80	26 13·5	380	40 5·0	51·5
90	41·2	390	32·0	50·8
100	27 9·0	400	41 0·2	51·2
			Mean	13 52·83
				= 832·83
			Rate of Chronometer	— ·02
			Time of 300 Vibrations	= 832·81

REDUCTION OF THE INVER-BAGASTY OBSERVATIONS.

CYLINDER, No. I. First Arc 40°.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829. July 23.	H. M. 8 10 P. M.	+ 9°·85	833″·02	+ 1″·066	— 0″·968	833″·118	852″·05
...	8 31 P. M.	+ 8·8	832·18	+ 1·390	— 0·968	832·580	851·50

OBSERVATIONS AT BONAR BRIDGE.

The Observations were made near the Kincardine Inn.

CYLINDER, No. I. First Arc 40°.

1829, July 24. A. M. Chronometer gaining + 2″·6.				Ther. + 14°.2. + 15.3. Mean + 14·75.
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 10 35 7·0	300	H. M. S. 10 49 3.2	M. S. 13 56·2
10	35·4	310	32·0	56·6
20	36 3·5	320	59·6	56·1
30	32·5	330	50 27·5	55·0
40	37 0·4	340	55·2	54·8
50	28·2	350	51 23·4	55·2
60	56·5	360	50·5	54·0
70	38 24·4	370	52 18·3	53·9
80	52·2	380	46·0	53·8
90	39 20·2	390	53 13·5	53·3
100	48·0	400	41·4	53·4
			Mean	13 54·754
			=	834·754
		Rate of Chron.	—	·024
		Time of 300 Vibs.	=	834·73

CYLINDER, No. I. First Arc 40°.

1829, July 24. A. M. Chronometer gaining + 2″·5.				Ther. + 15°·3 + 16·5 Mean, + 15·9
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 10 57 29·2	300	H. M. S. 11 11 25·5	M. S. 13 56·3
10	57·6	310	54·2	56·6
20	58 25·5	320	12 22·0	56.5
30	54·6	330	49·8	56·2
40	59 22·5	340	13 17·8	55·3
50	50·3	350	45·6	55·3
60	11 0 18·3	360	14 12·5	54·2
70	46·5	370	40·3	53·8
80	1 14·3	380	15 8·0	53·7
90	42·2	390	35·6	53·4
100	2 10·2	400	16 3·5	53·3
			Mean	13 55·054
			=	835·054
		Rate of Chron.	—	0·24
		Time of 300 Vibs.	=	835·03

REDUCTION OF OBSERVATIONS MADE AT BONAR BRIDGE.

Cylinder, No. I. First Arc 40°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 30°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 24.	H. M. 10 43 A. M.	+ 14°·75	834″·73	— 0″·45	— 0″·97	833″·31	852″·24
—	11 6 A. M.	+ 15·9	835·03	— 0·80	— 0·97	833·26	852·19

OBSERVATIONS AT INVERNESS.

The Observations were made in a Field near the north end of the Town, on the Dingwall Road.

Cylinder, No. I. First Arc 30°.

1829, July 26. P. M. Chronometer gaining + 2″·5. Ther. + 13°·3, + 12·6; Mean + 12·95

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 6 0 31·3	300	H. M. S. 6 14 21·0	M. S. 13 49·7
10	1 9·0	310	47·8	48·8
20	27·5	320	15 16·0	48·5
30	54·8	330	43·7	48·9
40	2 23·6	340	16 10·5	46·9
50	51·2	350	38·7	47·5
60	3 19·4	360	17 6·0	46·6
70	47·0	370	33·3	46·3
80	4 15·0	380	18 0·4	45·4
90	42·7	390	28·0	45·3
100	5 10·3	400	55·3	45·0
			Mean	13 47·173
			=	827·173
			Rate of Chronometer, —	·023
			Time of 300 Vibrations, =	827·15

Cylinder, No. I. First Arc 30°.

1829, July 26. P. M. Chronometer gaining + 2″·5. Ther. + 12°·6, + 12·0; Mean + 12·3

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 6 20 50·5	300	H. M. S. 6 34 39·7	M. S. 13 49·2
10	21 19·0	310	35 7·6	48.6
20	46·5	320	35·3	48·8
30	22 14·8	330	36 2·6	47·8
40	42·6	340	29·7	47·1
50	23 10·2	350	57·0	46·8
60	38·4	360	37 24·7	46·3
70	24 6·0	370	52·3	46·3
80	34·0	380	38 20·0	46·0
90	25 2·0	390	47·0	45·0
100	29·0	400	39 14·6	45·6
			Mean	13 47·045
			=	827·045
			Rate of Chronometer, —	·025
			Time of 300 Vibrations, =	827·02

OBSERVATIONS AT INVERNESS,—*continued.*

Flat Needle. First Arc 20°.

1829, July 26. P. M. Chronometer gaining + 2″·5.				Ther. + 12°·3 + 11·8 Mean + 12·05
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 44 15·4	300	7 1 53·5	17 38·1
10	50·5	310	28·5	38·0
20	26·8	320	4·0	37·2
30	1·0	330	39·3	38·3
40	37·2	340	14·3	37·1
50	12·8	350	49·3	36·5
60	48·0	360	24·5	36·5
70	23·4	370	0·0	36·6
80	58·5	380	35·3	36·8
90	34·6	390	10·3	35·7
100	9·0	400	45·5	35·5
			Mean	17 36·90
				= 1056·90
	Rate of Chronometer			— ·08
	Time of 300 Vibrations			= 1056·87

Flat Needle. First Arc 20°.

1829, July 26. P. M. Chronometer gaining + 2″·5.				Ther. + 11°·8 + 11·3 Mean + 11·55
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 11 40·2	300	7 29 18·2	17 38·0
10	15·8	310	53·2	37·4
20	51·0	320	28·8	37·8
30	27·0	330	4·2	37·2
40	2·0	340	39·0	37·0
50	37·5	350	13·8	36·3
60	12·7	360	49·2	36·5
70	48·3	270	24·7	36·5
80	23·5	380	59·2	35·7
90	59·5	390	35·0	35·5
100	34·5	400	10·0	35·5
			Mean	17 36·67
				= 1056·67
	Rate of Chronometer			— ·03
	Time of 300 Vibrations			= 1056·64

REDUCTION OF OBSERVATIONS MADE AT INVERNESS.

Cylinder, No. I. First Arc 30°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	H. M.	°	″	″	″	″	″
July 26.	6 9 P. M.	+ 12·95	827·15	+ 0·11	— 0·52	826.74	845·53
—	6 29 P. M.	+ 12·3	827·02	+ 0·31	— 0·52	826·81	845·61
Flat Needle. First Arc 20°.							
	H. M.	°	″	″		″	
July 26.	6 56 P. M.	+ 12·05	1056·87	+ 0·39	......	1057·26	
—	7 24 P. M.	+ 11·55	1056·64	+ 0·54	......	1057·18	

Log reduction of the Flat Needle to Cylinder No. I. = — 0·1067781.

OBSERVATIONS AT FORT AUGUSTUS.

Within the Fort, on the North-East Battery.

Cylinder, No. I. First Arc 30°.

1829, July 27. A. M. Chronometer gaining + 2″·5.				Ther. + 14°·5 + 15·2 Mean + 14·85
No.	**Time.**	**No.**	**Time.**	**Time of 300 Vibrations.**
	h. m. s.		h. m. s.	m. s.
0	10 4 40·0	300	10 18 29·0	13 49·0
10	5 7·5	310	56.8	49·3
20	35·5	320	19 24·0	48·5
30	6 3·3	330	51·8	48·5
40	31·2	340	20 19·2	48·0
50	59·0	350	46·5	47·5
60	7 26·2	360	21 14·5	48·3
70	54·5	370	41·5	47·0
80	8 22·2	380	22 9·0	46.8
90	49·8	390	36·4	46·6
100	9 17·8	400	23 4·0	46·2
			Mean	13 47·80
				= 827·80
	Rate of Chronometer,			— ·02
	Time of 300 Vibrations,			= 827·78

Out of the Fort, distant about 300 or 400 yards.

Cylinder, No. I. First Arc 30°.

1829, July 27. A. M. Chronometer gaining + 2″·5.				Ther. + 15°·2 + 15·5 Mean + 15·35
No.	**Time.**	**No.**	**Time.**	**Time of 300 Vibrations.**
	h. m. s.		h. m. s.	m. s.
0	10 29 25·0	300	10 43 13·3	13 48·3
10	52·0	310	41·2	49·2
20	30 20·0	320	44 8·8	48·8
30	48·0	330	36·8	48·8
40	31 16·0	340	45 4·3	48·3
50	43·5	350	31·5	48·0
60	32 11·3	360	59·2	47·9
70	39·3	370	46 26·6	47·3
80	33 7·0	380	54·0	47·0
90	34·8	390	47 21·5	46·7
100	34 1·8	400	48·7	46·9
			Mean	13 47·90
				= 827·90
	Rate of Chronometer,			— ·02
	Time of 300 Vibrations,			= 827·88

REDUCTION of OBSERVATIONS made at FORT AUGUSTUS.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	h. m.	°	″	″	″	″	″
July 27.	10 13 A. M.	+ 14·85	827·78	— 0·48	— 0·52	826·78	845·56
—	10 38 A. M.	+ 15·35	827·88	— 0·63	— 0·52	826·73	845·52

OBSERVATIONS AT FORT-WILLIAM.

The Observations were made in the Old Burial Ground.

CYLINDER, No. I. First Arc 30°.

1829, July 27. P. M. Chronometer gaining + 2″·5. Ther. + 13°·0, + 12·8; Mean + 12·9

No	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 47 3·5	300	7 0 43·5	13 40·0
10	31·3	310	1 11·2	39·9
20	58·7	320	38·5	39·8
30	48 26·8	330	2 6·0	39·2
40	53·6	340	33·4	39·6
50	49 21·7	350	3 1·2	39·5
60	48·8	360	27·8	39·0
70	50 16·7	370	55·0	38·3
80	43·4	380	4 22·8	39·4
90	51 11·0	390	49·8	38·8
100	38·0	400	5 17·0	39·0

Mean 13 39·32
= 819 32·
Rate of Chron. — ·02
Time of 300 Vibs. = 819·32

CYLINDER, No. I. First Arc 30°.

1829, July 27. P. M. Chronometer gaining + 2″·5. Ther. + 12°·8, + 12·5; Mean + 12·65

No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 7 50·0	300	7 21 30·0	13 40·0
10	8 17·8	310	58·2	40·4
20	45·5	320	22 26·2	40·7
30	9 12·7	330	53·0	40·3
40	40·8	340	23 20·3	39·5
50	10 7·8	350	47·8	40·0
60	36·0	360	24 15·8	39·8
70	11 2·8	370	42·0	39·2
80	30·7	380	25 9·8	39·1
90	58·0	390	37·0	39·0
100	12 25·7	400	26 3·8	38·1

Mean 13 39·645
= 819·645
Rate of Chron. — ·025
Time of 300 Vibs. = 819·62

CYLINDER, No. I. First Arc 30°.

1829, July 29. P. M. Chronometer gaining + 2″·5. Ther. + 12°·5, + 12·0; Mean + 12·25

No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 30 14·8	300	7 43 56·0	13 41·2
10	42·2	310	44 23·5	41·3
20	31 10·0	320	50·5	40·5
30	38·0	330	45 18·0	40·0
40	32 5·6	340	45·2	39·4
50	33·0	350	46 12·0	39·0
60	33 0·2	360	40·0	39·8
70	27·5	370	47 7·3	39·8
80	55·0	380	34·8	39·8
90	34 22·6	390	48 1·4	38·8
100	49·8	400	28·7	38·9

Mean 13 39·86
= 819·86
Rate of Chron. — ·02
Time of 300 Vibs. = 819·84

FLAT NEEDLE. First Arc 20°.

1829, July 27. P. M. Chronometer gaining + 2″·5. Ther. + 12°·0, + 11·5; Mean + 11·75

No.	Time.	No	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 52 5·3	300	8 9 35·2	17 29·9
10	41·0	310	10 10·7	29·7
20	53 16·5	320	45·0	29·5
30	51·2	330	11 20·0	28·8
40	54 26·8	340	54·2	27·4
50	55 1·5	350	12 30·0	28·5
60	36·8	360	13 4·5	27·7
70	56 11·8	370	39·8	28·0
80	46·8	380	14 14·2	27·4
90	57 21·8	390	49·0	27·2
100	56·8	400	15 24·5	27·7

Mean 17 28·345
= 1048·345
Rate of Chron. — ·035
Time of 300 Vibs. = 1048·31

REDUCTION of the OBSERVATIONS made at FORT-WILLIAM.

Cylinder, No. I. First Arc 30°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. July 27.	H. M. 6 56 P. M.	+ 12°·9	819″·30	+ 0″·12	— 0″·52	818″·90	837·51
...	7 17 P. M.	+ 12·65	819·62	+ 0·20	— 0·52	819·3	837·92
..	7 42 P. M.	+ 12·25	819·84	+ 0·32	— 0·52	819·64	838·26
Flat Needle. First Arc 20°.							
July 27.	H. M. 8 2 P. M.	+ 11°·75	1048″·31	+ 0″·60	0″·0	1048″·91	

Log Reduction of the Flat Needle to Cylinder, No. I. = — 0·1073058.

OBSERVATIONS AT OBAN.

The Observations were made on the high grounds north-east of the Town.

Cylinder, No. I. First Arc 25°.

1829, July 28. A. M. Chronometer gaining + 2″·5.				Ther. + 18°·4 + 18·4 Mean + 18·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 10 50 0·2	300	H. M. S. 11 3 38·8	M. S. 13 38·6
10	28·0	310	4 6·0	38·0
20	55·5	320	33·0	37·5
30	51 22·8	330	5 0·0	37·2
40	50·4	340	27·5	37·1
50	52 17·8	350	54·4	36·6
60	45·0	360	6 22·2	37·2
70	53 11·8			
80	39·8		Mean	13 37·46
90	54 7·0			= 817·46
100	34·2		Rate of Chron.	— ·02
			Time of 300 Vibs.	= 817·44

Cylinder, No. I. First Arc 25°.

1829, July 28. A. M. Chronometer gaining + 2″·5.				Ther. + 18°·4 + 18·3 Mean + 18·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 11 17 42·0	300	H. M. S. 11 31 20·5	M. S. 13 38·5
10	18 9·0	310	47·7	38·7
20	36·7	320	32 14·8	38·1
30	19 3·8	330	41·5	37·7
40	32·0	340	33 9·2	37·2
50	58·7	350	36·0	37·3
60	20 26·8	360	34 2·8	36·0
70	53·5			
80	21 21·8		Mean	13 37·64
90	48·3			= 817·64
100	22 15·6		Rate of Chron.	— ·02
			Time of 300 Vibs.	= 817·62

OBSERVATIONS AT OBAN,—*continued.*

CYLINDER, No. I. First Arc 25°.

1829, July 28. A. M. Chronometer gaining + 2″·5 daily.				Therm. + 18°·3 + 18·5 Mean + 18·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	11 37 59·3	300	11 51 38·0	13 38·7
10	38 27·0	310	52 5·0	38·0
20	54·0	320	32·2	38·2
30	39 21·5	330	59·2	37·7
40	48·8	340	53 26·0	37·2
50	40 16·8	350	53·0	6·2
60	43·7	360	54 19·8	6·1
70	41 11·0			
80	38·4		Mean	13 37·444
90	42 5·8			= 817·444
100	33·0		Rate of Chron.	— 0·24
			Time of 300 Vibs.	= 817·42

REDUCTION OF THE OBSERVATIONS MADE AT OBAN.

CYLINDER, No. I. First Arc 25°.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's standard.
1829.	H. M.						
July 28.	10 58 A. M.	+ 18°·4	817″·46	— 1″·55	— 0″·43	815″·48	834·01
—	11 26 A. M.	+ 18·35	817·64	— 1·53	— 0·43	815·68	834·21
—	11 46 A. M.	+ 18·4	817·42	— 1·55	— 0·43	815·44	833·97

OBSERVATIONS AT INVERARY.

The Observations were made in a Field west of the Town.

Cylinder, No. 1. First Arc 25°.

1829, July 29. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 14°·5 + 15·2 Mean + 14·85
No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	8 28 57·8	300	8 42 34·8	13 37·0
10	29 25·2	310	43 2·0	36·8
20	52·3	320	29·3	37·0
30	30 20·0	330	56·3	36·3
40	47·0	340	44 23·5	36·5
50	31 14·5	350	50·8	36·3
60	41·5	360	45 17·5	36·0
70	32 9·0	370	44·5	35·5
80	36·2	380	46 12·0	35·8
90	33 3·5	390	39·5	36·0
100	31·0	400	47 6·5	35·5
			Mean	13 36·245
				= 816·245
			Rate of Chronometer	— ·025
			Time of 300 Vibrations	= 816·22

Cylinder, No. I. First Arc 25°.

1829, July 29. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 15°·2 + 18·3 Mean + 16·75
No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	8 48 39·8	300	9 2 17·6	13 37·8
10	49 7·4	310	44·8	37·4
20	35·0	320	3 12·0	37·0
30	50 1·8	330	39·0	37·2
40	29 8	340	4 6·0	36·2
50	57·0	350	33·4	36·4
60	51 24·2	360	5 0·5	36 3
70	51·3	370	27·8	36·5
80	52 18·2	380	55·2	37·0
90	46·0	390	6 22·7	36·7
100	53 13·0	400	6 49·0	36·0
			Mean	13 36·774
				= 816·774
			Rate of Chronometer	— ·024
			Time of 300 Vibrations	= 816.75

Flat Needle. First Arc 20°.

1829, July 29. A. M. Chronometer gaining 2″·5 daily.				Ther. + 18°·3 + 18·3 Mean + 18·3
No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	9 13 13·6	300	9 30 38·0	17 24·4
10	48·0	310	31 12·2	24·2
20	14 23·0	320	47·0	24·0
30	57 8	330	32 22·0	24·2
40	15 33·0	340	56·8	23·8
50	16 7·7	350	33 31·6	23 9
60	42·6	360	34 6·5	23·9
70	17 17·8	370	41·0	23·2
80	52·5	380	35 16·0	23·5
90	18 27·6	390	51·3	23·7
100	19 2·0	400	36 26·0	24·0
			Mean	17 23·9
				= 1043·9
			Rate of Chronometer	— ·03
			Time of 300 Vibrations	= 1043·87

Flat Needle. First Arc 20°.

1829, July 29. A. M. Chronometer gaining + 2″·5 daily.				Ther. + 18°·4 + 18·8 Mean + 18·6
No.	Time. (H. M. S.)	No.	Time. (H. M. S.)	Time of 300 Vibrations. (M. S.)
0	9 49 37·3	300	10 7 2·0	17 24·7
10	50 12·5	310	37.0	24·5
20	48·7	320	8 12·0	23·3
30	51 22·8	330	46 8	24·0
40	57·5	340	9 21·8	24·3
50	52 32·0	350	56·8	24·8
60	53 7·0	360	10 31·2	24·2
70	42 2	370	11 6·0	23·8
80	54 17·4	380	40·2	22·8
90	52·0	390	12 15·8	23·8
100	55 27·0	400	50·5	23·5
			Mean	17 23·97
				= 1043·97
			Rate of Chronometer	— ·03
			Time of 300 Vibrations	= 1043·94

REDUCTION OF THE INVERARY OBSERVATIONS.

CYLINDER, No. I. First Arc 25°.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduction to Arc 20°.	Red. Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's standard.
1829. July 29.	H. M. 8 37 A. M.	+ 14°85	816″22	— 0″47	— 0″43	815″32	833″84
—	8 57 A. M.	+ 16·75	816·75	— 1·04	— 0·43	815·28	833·80
FLAT NEEDLE. First Arc 20°.							
July 29.	9 25 A. M.	18·3	1043·87	— 1·54	0·0	1042·33	
—	10 2 A. M.	18·6	1043·94	— 1·63	0·0	1042·31	

Log. Reduction of the Flat Needle to Cylinder, No. I. = — 0·1066836.

OBSERVATIONS AT GREENOCK.

The Observations were made in the Field in which Mr HERON's Observatory stands, and about 120 yards distant from the Observatory.

CYLINDER, No. I. First Arc 25°

1829, July 29. P. M. Chronometer gaining + 2″·5. Ther. + 13°·0, + 12·8; Mean, + 12·9

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 6 54 20·3	300	H. M. S. 7 7 51·3	M. S. 13 31·0
10	47·5	310	8 18·8	31·3
20	55 15·0	320	45·5	30·5
30	41·8	330	9 12·4	30·6
40	56 9·2	340	39·8	30·5
50	46·5	350	10 6·0	29·5
60	57 3·5	360	33·0	29·5
70	31·0	370	11 0·5	29·5
80	57·8	380	27·5	29·7
90	58 25·0	390	54·0	29·0
100	52·0			

Mean 13 30·11 = 810·11
Rate of Chronometer — ·02
Time of 300 Vibrations = 810·09

CYLINDER, No. I. First Arc 25°.

1829, July 29. P. M. Chronometer gaining + 2″·5. Ther. + 12°·8, + 11·4; Mean, + 12·1

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 7 16 4·3	300	H. M. S. 7 29 36·2	M. S. 13 31·9
10	32·0	310	30 3·0	31·0
20	59·5	320	30·0	30·5
30	17 26·8	330	57.0	30·2
40	53·5	340	31 23·8	30·3
50	18 21·0	350	51·0	30·0
60	48·4	360	32 17·5	29·1
70	19 15·7	370	44·8	29·1
80	42·8	380	33 11·8	29·0
90	20 9·4	390	39·0	29·6
100	36·8			

Mean 13 30·07 = 810·07
Rate of Chronometer — ·02
Time of 300 Vibrations = 810·05

OBSERVATIONS AT GREENOCK,—*continued.*

Cylinder, No. I. First Arc 25°.

1829, July 29. P. M. Chronometer gaining + 2''.5 daily.				Therm. + 11°·4 + 11.0 Mean, + 11·2
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 36 25·0	300	7 49 55·7	13 30·7
10	52·0	310	50 22·8	30·8
20	19·5	320	49·6	30·1
30	46·7	330	16·5	29·8
40	13·8	340	43·0	29.2
50	41·2	350	10·6	29·4
60	8·0	360	37·3	29·3
70	35·0	370	4·0	29·0
80	2·2	380	31·0	28·8
90	29·5	390	57·7	28·2
100	40 56·4	400	54 25·6	29·2
			Mean	13 29·5
				= 809·50
	Rate of Chronometer			— ·02
	Time of 300 Vibrations			= 809·48

REDUCTION of the OBSERVATIONS at GREENOCK.

Cylinder, No. I. First Arc 25°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829.	H. M.	°	''	''	''	''	''
July 29.	7 3 P. M.	+ 12·9	810·09	+ 0·12	— 0·42	809·79	828·19
—	7 25 P. M.	+ 12·1	810·05	+ 0·36	— 0·42	809·99	828·39
—	7 45 P. M.	+ 11·2	809·48	+ 0·63	— 0·42	809·69	828·09

OBSERVATIONS AT BRISBANE (near Largs.)

The Observations were made about 40 Yards South of the Observatory.

CYLINDER, No. I. First Arc 25°.

1829, July 30. P. M. Chronometer gaining + 2″·5.				Ther. + 17°·2 + 17·0 Mean + 17·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 9 15·0	300	4 22 51·5	13 36·5
10	42·5	310	23 19·2	36·7
20	9·8	320	46·2	36·4
30	37·0	330	24 13·0	36·0
40	4·5	340	40·5	36.0
50	31·8	350	25 7·4	35·6
60	59·0	360	33·8	34·8
70	26·5	370	26 1·5	35·0
80	53·5	380	29·0	35·5
90	21·0	390	56·0	35·0
100	48·3	400	27 23·2	34·9
			Mean	13 35·673
				= 815·673
	Rate of Chronometer			— ·023
	Time of 300 Vibrations			= 815·65

CYLINDER, No. I. First Arc 25°.

1829, July 30. P. M. Chronometer gaining + 2″·5.				Ther. + 17°·0 + 17·4 Mean + 17·2
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	4 30 53·5	300	4 44 30·0	13 36·5
10	21·0	310	57·2	36·2
20	48·5	320	45 24·5	36·0
30	16·2	330	51·8	35·6
40	43·0	340	46 19·2	36·2
50	11·0	350	46·0	35·0
60	38·0	360	47 13·6	35·6
70	5·0	370	40·8	35·8
80	32·4	380	48 8·0	35·6
90	59·5	390	35·0	35·5
100	27·0	400	49 2·2	35·2
			Mean	13 35·754
				= 815·754
	Rate of Chronometer			— ·024
	Time of 300 Vibrations			= 815·73

REDUCTION OF THE BRISBANE OBSERVATIONS.

CYLINDER, No. I. First Arc 25°.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibs.	Reduction to + 13°·3.	Reduction to Arc 20°.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829. July 30.	H. M. 4 18 P. M.	+ 17°·1	815″·65	— 1″·15	— 0″·43	814″·07	832″·57
—	4 39 P. M.	+ 17·2	815·73	— 1·18	— 0·43	814·12	832·62

OBSERVATIONS AT AYR.

The Observations were made on the Green.

Cylinder, No. I. First Arc 20°.

1829, August 3. A. M. Chronometer gaining + 2″·5.				Ther. + 14°·0 + 16·0 Mean + 15·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	11 52 1·2	300	12 5 22·0	13 20·8
10	28·2	310	48·5	20·3
20	55·0	320	6 15·5	20·5
30	53 22·0	330	42·2	20·2
40	48·5	340	7 8·8	20·3
50	54 15·5	350	35·5	20·0
60	42·5	360	8 2·0	19·5
70	55 8·6			
80	35·8		Mean	13 20·23
90	56 1·2			= 800·23
100	29·0	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 800·21

Cylinder, No. I. First Arc 20°.

1829, August 3. P. M. Chronometer gaining + 2″·5.				Ther. + 16°·0 + 18·5 Mean + 17·25
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	12 11 13·5	300	12 24 34·7	13 21·2
10	40·8	310	25 1·4	20·6
20	7·2	320	28·2	21·0
30	34·5	330	54·8	20·3
40	0·8	340	26 21·5	20·7
50	27·8	350	48·3	20·5
60	54·0	360	27 15·0	21·0
70	21.6			
80	48·0		Mean	13 20·759
90	14·4			= 800·759
100	41·5	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 800.74

REDUCTION of the OBSERVATIONS at AYR.

Cylinder No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. Aug. 3.	H. M. 12 0 noon.	+ 15°·0	800″·21	— 0″·50	799″·71	817″·88
—	0 19 P. M.	+ 17·25	800·74	— 1·17	799·57	817·74

OBSERVATIONS AT SANQUHAR.

The Observations were made in the Rev. Mr SIMPSON's Garden.

CYLINDER, No. I. First Arc 20°.

1829, August 5. A. M. Chronometer gaining + 2″·5.				Ther. + 12°·3 + 12·4 Mean, + 12·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 36 5·2	300	7 49 32·6	13 27·4
10	32·5	310	59·5	27·0
20	59·0	320	50 26·6	27·6
30	37 26·5	330	53·2	26·7
40	53·0	340	51 19·5	26·5
50	38 20·5	350	46·8	26·3
60	47·3	360	52 13·7	26·4
70	39 14·3			
80	41·2		Mean	13 26·843
90	40 8·3			= 806·843
100	35·3	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 806·823

CYLINDER, No. I. First Arc 20°.

1829, August 5. A. M. Chronometer gaining + 2″·5.				Ther. + 12°·4 + 12·4 Mean, + 12·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 55 55·5	300	8 9 23·4	13 27·9
10	56 22·5	310	50·2	27·7
20	49·8	320	10 17·0	27·2
30	57 17·0	330	43·6	26·6
40	44·2	340	11 10·4	26·2
50	58 11·0	350	37·7	26·7
60	38·0	360	12 4·4	26·4
70	59 4·8			
80	32·0		Mean	13 26·957
90	58·8			= 806·957
100	8 0 26·3	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 816·937

FLAT NEEDLE. First Arc 20°.

1829, August 5. A. M. Chronometer gaining + 2″·5.				Ther. + 12°·4 + 12·4 Mean, + 12·4
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 19 57·8	300	8 37 13·0	17 15·2
10	20 32·8	310	47·0	14·2
20	21 7·2	320	38 21·8	14·6
30	42·0	330	56·5	14·5
40	22 16·8	340	39 31·0	14·2
50	51·0	350	40 5·0	14·0
60	23 25·8	360	40·0	14·2
70	24 0·2	370	41 13·6	13·4
80	35·0	380	48·3	13·3
90	25 9·0	390	42 22·8	13·8
100	43·8	400	57·0	13·2
			Mean	17 14·054
				= 1034·054
	Rate of Chronometer			— ·034
	Time of 300 Vibrations			= 1034·02

REDUCTION of the SANQUHAR OBSERVATIONS.

Cylinder, No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Therm.	Time of 300 Vibrations.	Reduction to + 13°·3 R.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. Aug. 5.	H. M. 7 44 A. M.	+ 12°·35	806″·823	+ 0″·28	807″·103	825″·44
—	8 4 A. M.	+ 12·4	806·937	+ 0·27	807·207	825·55
Flat Needle. First Arc 20°.						
Aug. 5.	H. M. 8 31 A. M.	+ 12°·4	1034″·02	+ 0″·34	1034″·36	

Log Reduction of the Flat Needle to Cylinder No. I. = — 0·1077152.

OBSERVATIONS AT DUMFRIES.

The Observations were made in a Garden on Mount Pleasant.

Cylinder, No. I. First Arc 20°.

1829, August 5. P. M. Chronometer gaining + 2″·5. Ther. + 13°·5, + 13.3; Mean + 13·4

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 7 12 30·5	300	H. M. S. 7 25 61·5	M. S. 13 21·0
10	56·5	310	26 18·0	21·5
20	13 23·5	320	44·8	21·3
30	49·8	330	27 11·3	21·5
40	14 17·5	340	38·0	20·5
50	44·0	350	28 4·7	20·7
60	15 10·8	360	30·8	20 0
70	48·0			
80	16 4·0		Mean	13 20·93
90	31·2			= 800·93
100	57·7		Rate of Chron.	— .02
			Time of 300 Vibs.	= 800·91

Cylinder, No I. First Arc 20°.

1829, August 5. P. M. Chronometer gaining + 2″·5. Ther. + 13°·3, + 13·0; Mean + 13·15

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 7 31 7·8	300	H. M. S. 7 44 29·0	M. S. 13 21·2
10	34·8	310	55·8	21·0
20	32 1·0	320	45 22·4	21·4
30	28·4	330	48·6	20·2
40	55·0	340	46 15·5	20·5
50	33 21·8	350	42·0	20·2
60	48·6	360	47 8·6	20·0
70	34 15·8			
80	42·8		Mean	13 20·643
90	35 9·0			= 800·643
100	35·2		Rate of Chron.	— ·020
			Time of 300 Vibs.	= 800·623

OBSERVATIONS AT DUMFRIES,—*continued.*

CYLINDER, No. I. First Arc 20°.

1829, August 5. P. M. Chronometer gaining + 2″·5.				Ther. + 13°·0 + 12·5 Mean + 12·75
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 50 45·7	300	8 4 6·8	13 21·1
10	51 12·0	310	33·8	21·8
20	39·8	320	5 0·4	20·6
30	52 6·2	330	26·8	20·6
40	33·0	340	53·5	20·5
50	59·7	350	6 20·5	20·8
60	53 26·8	360	47·2	20·4
70	53·2			
80	54 19·8		Mean	13 20·83
90	46·8			= 800·83
100	55 13·2	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 800·81

REDUCTION OF THE DUMFRIES OBSERVATIONS.

CYLINDER, No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13.°3 R.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's standard.
1829.	H. M.					
Aug 5.	7 20 P. M.	+ 13°·4	800″·91	— 0″·03	800″·88	819″·08
—	7 39 P. M.	+ 13·15	800·62	+ 0·05	800·67	818·87
—	7 59 P. M.	+ 12·75	800·81	+ 0.16	800.97	819·17

OBSERVATIONS AT ANNAN.

The Observations were made in the Rev. Mr Dobie's Garden.

Cylinder, No. I. First Arc 20°.

1829, August 6. P. M. Chronometer gaining + 2″·5.				Ther. + 20°·2, + 22·2; Mean + 21·2
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	0 0 48·4	300	0 14 14·0	13 25·6
10	1 16·0	310	41·0	25·0
20	42·6	320	15 8·0	25·4
30	2 9·8	330	35·0	25·2
40	36·8	340	16 1·2	24·4
50	3 3·2	350	28·0	24·8
60	31·0	360	55·0	24·0
70	57·0			
80	4 24·8		Mean	13 24·485
90	51·0			= 804·485
100	5 18·0		Rate of Chron.	— ·02
			Time of 300 Vibs.	= 804·465

Flat Needle. First Arc 20°.

1829, August 6. P. M. Chronometer gaining + 2″·5				Ther. + 22°·2, + 20·4; Mean + 21·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	0 22 27·5	300	0 39 40·5	17 13·0
10	23 2·0	310	40 14·7	12·7
20	36·7	320	49·0	12·3
30	24 10·8	330	41 23·0	12·2
40	45·0	340	57·3	12·3
50	25 20·0	350	42 31·4	11·4
60	54·0	360	43 5·7	11·7
70	26 29·0	370	39·8	10·8
80	27 2·8	380	44 14·0	11·2
90	37·7	390	48·0	10·3
100	28 12·2	400	45 22·2	10·0
			Mean	17 11·627 =1031·627
			Rate of Chron.	— ·027
			Time of 300 Vibs.	=1031·6

REDUCTION of the ANNAN OBSERVATIONS.

Cylinder No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's standard.
1829. Aug. 6.	H. M. 0 9 P. M.	+ 21°·2	804″·465	— 2″·355	802″·11	820″·33
		Flat Needle. First Arc 20°.				
Aug. 6.	0 34 P. M.	+ 21·3	1031·6	— 2·89	1028·71	

Log. Reduction of Flat Needle to Cylinder, No. I. = —0·1080591.

OBSERVATIONS AT CARLISLE.

The Observations were made on the Common Green, East of the Bridge.

CYLINDER, No. I. First Arc 20°.

1829, August 6. P. M. Chronometer gaining + 2″·5.				Ther. + 14°·2, + 13·8; Mean + 14·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 19 7·5	300	6 32 28·3	13 20·8
10	34·8	310	54·3	20·0
20	20 1·2	320	33 21·6	20·4
30	28·3	330	48·0	19·7
40	55·0	340	34 14·8	19·8
50	21 21·8	350	41·5	19·7
60	48·2	360	35 8·0	19·8
70	22 15·3			
80	41·6		Mean	13 20·028
90	23 8·6			= 800·028
100	35·6	Rate of Chron.		— ·020
		Time of 300 Vibs.		= 800·008

CYLINDER, No. I. First Arc 20°.

1829, August 6. P. M. Chronometer gaining + 2″·5.				Ther. + 13°·8, + 13·4; Mean + 13·6
No.	Time.	No.	Time	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 39 0·6	300	6 52 21·3	13 20·7
10	27·8	310	47·8	20·0
20	54·5	320	53 14·5	20·0
30	40 21·3	330	41·2	19·9
40	48·0	340	54 7·7	19·7
50	41 14·8	350	34·5	19·7
60	41·7	360	55 0·8	19·1
70	42 8·2			
80	35·2		Mean	13 19·870
90	43 1·8			= 799·87
100	29·0	Rate of Chron.		— ·02
		Time of 300 Vibs.		= 799·85

FLAT NEEDLE. First Arc 20°.

1829, August 6. P. M. Chronometer gaining + 2″·5.				Ther. + 13°·4, + 13·2; Mean + 13·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	6 59 17·0	300	7 16 23·0	17 6·0
10	50·8	310	56·6	5·8
20	26·0	320	17 31·2	5·2
30	0·0	330	18 5·0	5·0
40	35·0	340	39·8	4·8
50	8·8	350	19 13·0	4·2
60	42·8	360	47·5	4·7
70	17·0	370	20 21·5	4·5
80	51·2	380	56·0	4·8
90	26·3	390	21 30·8	4·5
100	59·7	400	224 ·0	4·3
			Mean	17 4·89
				=1024·89
			Rate of Chronometer	— ·03
			Time of 300 Vibrations	=1024·86

REDUCTION OF THE CARLISLE OBSERVATIONS.

Cylinder, No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's standard.
1829. Aug. 6.	H. M. 6 27 P. M.	+ 14°·0	800″·008	— 0″·208	799″·80	817″·97
—	6 47 P. M.	+ 13·6	799·850	— 0·09	799·76	817·93
			Flat Needle. First Arc 20°.			
Aug. 6.	7 12 P. M.	+ 13·3	1024·86	0·00	1024·86	

Log Reduction of the Flat Needle to Cylinder No. I. = — 0·1076942.

OBSERVATIONS AT HOLY ISLAND.

The Observations were made within the Ruins of the Old Abbey.

Cylinder, No. I. First Arc. 20°.

1829, September 1. A. M. Chronometer gaining + 4″·2 daily. Ther. + 22°·2, + 24·2, Mean + 23·2

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 11 27 20·2	300	H. M. S. 11 40 48·0	M. S. 13 27·8
10	47·5	310	41 15·0	27·5
20	28 14·8	320	42·0	27·2
30	41·3	330	42 8·5	27·2
40	29 8·5	340	36·0	27·5
50	35·8	350	43 2·8	27·0
60	30 2·0	360	29·7	27·7
70	29·8			
80	56·0		Mean	13 27·414
90	31 23·5			= 807·414
100	50·2		Rate of Chron.	— ·035
			Time of 300 Vibs.	= 807.38

Cylinder, No. I. First Arc 20°.

1829, September 1. A. M. Chronometer gaining + 4″·2 daily. Ther. + 24°·2, + 24·5, Mean + 24·35

No.	Time.	No.	Time.	Time of 300 Vibrations.
0	H. M. S. 11 45 35·0	300	H. M. S. 11 59 3·0	M. S. 13 28·0
10	46 2·0	310	30·0	28·0
20	29·3	320	56·8	27·5
30	56·5	330	12 0 23·6	27·1
40	47 23·8	340	50·7	26·9
50	50·6	350	1 17·8	27·2
60	48 17·5	360	44·5	27·0
70	44·8			
80	49 11·6		Mean	13 27·386
90	38·3			= 807·386
100	50 5·0		Rate of Chron.	— ·035
			Time of 300 Vibs.	= 807·35

REDUCTION OF THE HOLY ISLAND OBSERVATIONS.

Year and Month.	Mean Time of the Observations.	REAUM. Thermom.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to HANSTEEN's Standard.
1829.	H. M.	°	"	"	"	"
Sept. 1.	11 35 A. M.	+ 23·2	807·38	— 2·97	804·41	822·69
—	11 54 A. M.	+ 24·35	807·35	— 3·14	804·21	822·48

OBSERVATIONS AT BERWICK-UPON-TWEED.

The Observations were made on the Magdalene Fields.

CYLINDER, No. I. First Arc 20°.

1829, September 2. A. M. Chronometer gaining + 4"·2 daily.				Ther. + 13°·0 + 13·0 Mean + 13·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 22 24·0	300	7 35 48·6	13 24·6
10	50·8	310	36 15·0	24·2
20	23 17·8	320	42·0	24·2
30	44·8	330	37 8·6	23·8
40	24 11·3	340	35·8	24·5
50	38·0	350	38 2·0	24·0
60	25 4·4	360	29·0	24·6
70	32·0			
80	59·0		Mean	13 24·27
90	26 25·8			= 804·27
100	52·8	Rate of Chron.		— ·035
		Time of 300 Vibs.		= 804·235

CYLINDER, No. I. First Arc 20°.

1829, September 2. A. M. Chronometer gaining + 4"·2 daily.				Ther. + 13°·0 + 13·7 Mean + 13·35
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	7 41 19·0	300	7 54 43·5	13 24·5
10	45·7	310	55 10·2	24·5
20	42 12·8	320	37·0	24·2
30	49·3	330	56 3·8	24·5
40	43 6·6	340	31·0	24·4
50	33·8	350	57·8	24·0
60	44 0·5	360	57 24·6	24·1
70	27·7			
80	53·8		Mean	13 24·314
90	45 21·2			= 804·314
100	47·8	Rate of Chron.		— ·035
		Time of 300 Vibs.		= 804·28

OBSERVATIONS AT BERWICK-UPON-TWEED,—*continued.*

Flat Needle. First Arc 20°.

1829, September 2. A. M. Chronometer gaining + 4″·2 daily.				Ther. + 13°·7 + 14·3 Mean + 14·0
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	S. M.
0	8 5 29·0	300	8 22 43·0	17 14·0
10	6 3·5	310	23 17·0	13·5
20	38·0	320	51·8	13·8
30	7 12·7	330	24 26·0	13·3
40	47·6	340	25 0·4	12·8
50	8 22·0	350	34·8	12·8
60	56·4	360	26 9·4	13·0
70	9 31·2	370	44·0	12·8
80	10 5·2	380	27 18·0	12·8
90	39·8	390	52·5	12·7
100	11 15·0	400	28 27·5	12·5
			Mean	17 13·09
				= 1033·09
			Rate of Chronometer	— ·05
			Time of 300 Vibrations	= 1033·04

REDUCTION of BERWICK-UPON-TWEED OBSERVATIONS.

Cylinder, No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Therm.	Time of 300 Vibrations.	Reduction to + 13°·3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. Sept. 2.	H. M. 7 30 A. M.	+ 13°·0	804″·235	+ 0″·085	804″·32	822″·60
—	7 49 A. M.	+ 13·35	804·280	— 0·02	804·26	822·53
Flat Needle. First Arc 20°.						
Sept. 2.	H. M. 8 17 A. M.	+ 14°·0	1033″·04	— 0″·27	1032″·77	

Log Reduction of the Flat Needle to Cylinder, No. I. = — 0·1085912.

OBSERVATIONS ON THE TOP OF THE CHEVIOT HILLS.

The Observations were made at the foot of the Flag-staff.

Cylinder, No. I. First Arc 20°.

1829, September 18. A. M. Chronometer gaining + 3″·4 daily.				Ther. + 6°·2 + 6·0 Mean + 6·1
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 35 51·0	300	8 49 14·5	13 23·5
10	36 17·5	310	41·4	23·9
20	45·0	320	50 8·2	23·2
30	37 12·0	330	34·8	22·8
40	39·0	340	51 1·8	22·8
50	38 5·5	350	28·2	22·7
60	32·5	360	54·8	22·3
70	59·2			
80	39 26·4		Mean	13 23·027
90	53·2			= 803·027
100	40 20·0		Rate of Chron.	— ·027
			Time of 300 Vibs.	= 803·00

Cylinder, No. I. First Arc 20°.

1829, September 18. P. M. Chronometer gaining + 3″·4 daily.				Ther. + 6°·0 + 6·6 Mean + 6·3
No.	Time.	No.	Time.	Time of 300 Vibrations.
	H. M. S.		H. M. S.	M. S.
0	8 54 58·5	300	9 8 22·2	13 23·7
10	55 25·0	310	48·8	23·8
20	52·4	320	9 15·7	23·3
30	56 19·6	330	42·2	22·6
40	46·5	340	10 9·3	22·8
50	57 13·0	350	35·7	22·7
60	39·8	360	11 2·3	22·5
70	58 6·6			
80	33·8		Mean	13 23·057
90	59 0·7			= 803·057
100	27.5		Rate of Chron.	— ·027
			Time of 300 Vibs.	= 803·03

REDUCTION OF THE CHEVIOT HILL OBSERVATIONS.

Cylinder, No. I. First Arc 20°.

Year and Month.	Mean Time of the Observations.	Reaum. Thermom.	Time of 300 Vibrations.	Reduction to + 13°.3.	Reduced Time of 300 Vibs.	Reduced Time of 300 Vibrations reduced to Hansteen's Standard.
1829. Sept. 18.	H. M. 8 44 A. M.	+ 6°·1	803″·00	+ 2″·14	805″·14	823″·43
...	9 9 A. M.	+ 6·3	803·03	+ 2·08	805·11	823·40

By comparing the Observations made at Makerstoun, from January 1829 to January 1830, it appears that the magnetic intensity of Cylinder, No. I. has remained stationary; whereas that of the *Flat Needle* has been decreasing, which will be seen by comparing the log. reduction of the Observations made in Christiania in 1827, and at Makerstoun in 1829 and 1830: and, that its magnetism has not yet become stationary will also appear by comparing the log. reduction of the Observations made at the different stations to No. I. For which reason, I have not connected the Observations by the Flat Needle with those of Cylinder, No. I.

The following Table will show the log. reduction of the Flat Needle to Cylinder, No. I. at the different stations where Observations have been made with both needles:

LOG. REDUCTION OF THE FLAT NEEDLE TO CYLINDER, NO. I.

		Log. Reduction.
Christiania, June 1827,		— 0·0882541
Makerstoun,	December and January 1828–9,	— 0·0934693
	June 1829,	— 0·0954164
	January and February 1830,	— 0·1020146
Edinburgh,	Captain Hall's Garden (1829),	— 0·1032437
	Canaan Cottage (July),	— 0·1062819
Fort-George,		— 0·1054496
Tain,		— 0·1062040
Dornoch,		— 0·1066942
Thurso,		— 0·1051618
Tongue,		— 0·1065578
Inverness,		— 0·1067781
Fort-William,		— 0·1073058
Inverary,		— 0·1066836
Sanquhar,		— 0·1077152
Annan,		— 0·1080591
Carlisle,		— 0·1076942
Berwick,		— 0·1085912

TABLE

CONTAINING THE RESULT OF THE OBSERVATIONS WITH CYLINDER No. I.

Names of Places where the Observations were made.	North Latitude.	Longitude from Edinburgh.	Hour of the Day at which the Observations were made.	Time of 300 Vibrations reduced to Hansteen's Standard Cylinder (by Dollond.)	Reduction to 2h 30' P.M. or to the Mean of the Day.	Time of 300 Vibrations corrected to the Mean of the Day.	Horizontal Intensity at the different stations, Edinburgh taken as unity.
	° '	° '		"	"	"	1·02084
Christiania, 1827	...	...	A. M. & P. M.	819·17	0·00	819·17	1·02084
Makerstoun, 1829	55·36	0·42 E.	A. M. & P. M.	822·20	0·00	822·20	1·01332
Edin. Capt. Hall's	...	...	8 48 A. M.	817·0	— 0·65	816·35	1·02814
Edin. Cannan Cottage	55·58	...	A. M. & P. M.	827·87	— 0·21	827·66	1·00000
Dundee	56·27	0·14 E.	7 32 P. M.	834·94	+ 0·85	835·79	·98064
Brechin	56·44	0·31 E·	10 33 A. M.	836·09	— 0·79	835·30	·98180
Montrose	56·43	0·46 E·	2 53 P. M.	833·52	+ 0·08	833·60	·98580
Aberdeen	57·9	1·3 E·	4 57 & 7 52 P. M.	839·65	+ 0·60	840·25	·97026
Banff	57·40	0·39 E.	4 46 P. M.	841·47	+ 0·39	841·86	·96434
Portsoy	57·42	0·26 E.	9 40 A. M.	836·73	— 0·80	836·07	·97998
Elgin	57·39	0·8 W.	8 45 A. M.	845·80	— 0·65	845·15	·95904
Forres	57·37	0·25 W.	2 49 P. M.	843·61	+ 0·05	843·66	·96243
Nairn	57·36	0·38 W.	8 25 P. M.	841·40	+ 0·60	842·00	·96623
Fort George	57·36	0·43 W.	12 18 P. M.	845·41	— 0·45	844·96	·95969
Tain	57·50	0·51 W.	2 22 P. M.	851·10	0·00	851·10	·94599
Dornoch	57·55	0·53 W.	8 49 A. M.	851·95	— 0·65	851·30	·94523
Helmsdale	58·9	0·32 W.	5 4 P. M.	852·33	+ 0·52	852·85	·94180
Wick	58·27	0·4 E.	5 45 P. M.	854·35	+ 0·65	855·00	·93707
Thurso	58·36	0·24 W.	11 0 A. M.	854·69	— 0·69	854·00	·93927
Tongue	58·29	1·12 W.	A. M. & P. M.	854·78	0·00	854·78	·93755
Inver-Bagasty	58·17	1·14 W.	8 20 P. M.	851·78	+ 0·62	852·40	·94280
Bonar Bridge	57·57	1·12 W.	10 54 A. M.	852·21	— 0·71	851·50	·94479
Inverness	57·29	1·1 W.	6 19 P. M.	845·57	+ 0·76	846·33	·95637
Fort Augustus	57·9	1·28 W.	10 25 A. M.	845·54	— 0·80	844·74	·95990
Fort William	56·50	1·56 W.	7 19 P. M.	837·89	+ 0·84	838·73	·97378
Oban	56·26	2·18 W.	11 22 A. M.	834·06	— 0·62	833·44	·98618
Inverary	56·15	1·51 W.	8 47 A. M.	833·82	— 0·62	833·20	·98675
Greenock	55·57	1·33 W.	7 24 P. M.	828·22	+ 0·82	829·04	·99667
Brisbane	55·49	1·39 W.	4 28 P. M.	832·60	+ 0·38	832·98	·98727
Ayr	55·25	1·15 W.	12 10 P. M.	817·81	— 0·46	817·35	1·02538
Sanquhar	55·21	0·45 W.	7 54 A. M.	825·50	— 0·50	825·00	1·00646
Dumfries	55·3	0·24 W.	7 39 P. M.	819·04	+ 0·77	819·81	1·01600
Annan	54·59	0·2 W.	12 9 P. M.	820·33	— 0·47	819·86	1·01912
Carlisle	54·54	0·25 E.	6 37 P. M.	817·95	+ 0·82	818·77	1·02183
Holy Island	55·40	1·20 E.	11 45 A. M.	822·58	— 0·55	822·03	1·01374
Berwick	55·47	1·9 E.	7 40 A. M.	822·56	— 0·44	822·12	1·01352
Cheviot	55·30	1·2 E.	8 54 A. M.	823·41	— 0·68	822·73	1·01202

Notice concerning an Autograph Manuscript by Sir ISAAC NEWTON, *containing some Notes upon the Third Book of the Principia, and found among the Papers of Dr* DAVID GREGORY, *formerly Savilian Professor of Astronomy in the University of Oxford.* By JAMES CRAUFURD GREGORY, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

(Read March 2. 1829.)

AN opinion has been entertained by some of the modern French philosophers, and, among others, by the late celebrated Marquis de LA PLACE, that it was only when far advanced in years that Sir ISAAC NEWTON turned his attention to the study of Theology; and it has been lately assumed by M. BIOT, in an account of NEWTON and his discoveries, contained in the " Biographie Universelle," as a fact which can scarcely be doubted, that, at one period of his life, he was actually in a state of mental derangement.

The only evidence adduced in support of this remarkable assertion is the following note, said to have been written by HUYGENS, and communicated to M. BIOT by M. VAN SWINDEN: " *Die* 29 *Maii* 1694.—Narravit mihi D. COLIN, Scotus, virum celeberrimum ac rarum geometram ISAACUM NEWTONUM incidisse in phrenitin abhinc anno et sex mensibus. An ex nimia studii assiduitate, an dolore infortunii, quòd in incendio, laboratorium chemicum et scripta quædam amiserat? Cùm ad Archiepisco-

pum Cantabrigiensem venisset, ea locutum quæ alienationem mentis indicarent; deinde ab amicis cura ejus suscepta, domoque clausa, remedia volenti nolenti adhibita, quibus jam sanitatem recuperavit, et jam nunc librum suum Principiorum intelligere incipiat." Upon this note the following observations are made by M. Biot :—" Il parait d'après ces détails que l'on ne saurait guère douter du fait même, c'est à dire, que cette tête qui pendant tant d'années s'etait appliquée continument à des contemplations si profondes quelles étaient comme la dernière limite de la raison humaine, se serait enfin troublée elle-même par l'excès de ses efforts, ou par la douleur d'en voir les résultats anéantis."

The misfortune here alluded to by Huygens and M. Biot, is the well known anecdote of the loss caused by his dog Diamond; and it is to this circumstance principally that M. Biot ascribes the mental derangement under which he supposes Newton to have laboured. He adds, " Mais ce fait, d'un dérangement d'esprit, quelle qu'en puisse être la cause, expliquerait pourquoi, depuis la publication du livre des Principes en 1687, Newton agé seulement alors de 45 ans, n'a plus donné de travail nouveau sur aucune partie des sciences, et s'est contenté de faire connaitre ceux qu'il avait composé long-temps avant cette epoque, en se bornant à les compléter dans les parties qui pouvaient avoir besoin de developpemens." M. Biot supposes, that after this period of Newton's life, he almost ceased to think on scientific subjects, and that religious reading formed his most habitual occupation; and he states, that after the " fatal epoch," as he terms it, of 1693, only three really new scientific productions appeared from his hand, one of which had been probably prepared for a long time previously, and the others had cost him very little time.

It is not the object of the present notice to disprove the assertion that Sir Isaac Newton was at one period of his life in a

state of mental derangement; but to shew on what slender grounds this allegation rests, it may be mentioned, that M. BIOT seems to find an argument in its favour in the magnanimity with which NEWTON bore the irreparable loss of the labour of many years! And, as a proof that his mental powers were not impaired, either by this accident, or by the advance of years, it may be sufficient to allude to the fact, admitted by M. BIOT himself, that, in the year 1716, and at the age of 74, NEWTON, on returning from his duties at the Mint, and when much fatigued with the business of the day, solved, before he retired to rest, the celebrated problem of Orthogonal Trajectories, proposed by LEIBNITZ with a view to prove the superiority of his calculus over NEWTON's method of fluxions, and, as he expresses himself, "ut pulsum Anglorum Analystarum nonnihil tentemus *."

As Sir ISAAC NEWTON's Observations on the Prophecies and the Apocalypse were not published during his lifetime, and as the celebrated general Scholium at the end of the Principia only appeared in the second edition of that work, published in 1713, when the author was in his 71st year, the supposition entertained by LA PLACE, that NEWTON only turned his attention to the subject of theology in his very advanced years, is somewhat more natural and plausible in the absence of any direct evidence to the contrary. So impressed was he with this idea, that M. GAUTIER, Professor of Astronomy in Geneva, mentioned, when in this country a few years ago, that he had been commissioned by LA PLACE to make inquiries on this subject. Any opinion entertained by this great man, by whose genius and labours the Newtonian Philosophy may be said to have been completed in all its details, is entitled to due respect and attention from all, and, in the minds of many, must carry with it considerable weight. A do-

* LEIBNITII et BERNOULLII Commercium Epistolicum, tom. ii. Epist. ccxxvi. p. 365.

cument in my possession, however, appears to me to furnish clear evidence that NEWTON had formed the theological opinions expressed in the Scholium already mentioned, at least fifteen years before the publication of the edition of the Principia, in which it first appeared; and it has occurred to me, that a short statement of this evidence, in opposition to the opinion entertained by LA PLACE, might not be unacceptable to the Royal Society.

Several years ago, in looking over some manuscript mathematical papers which belonged to DAVID GREGORY, Savilian Professor of Astronomy in the University of Oxford, the contemporary and intimate friend of NEWTON, I found (along with several other autograph fragments on mathematical subjects), one manuscript, consisting of twelve folio pages, in the handwriting of NEWTON, and containing, in the form of additions and scholia to some propositions in the Third Book of the Principia, an account of the opinions of the ancient philosophers on gravitation and motion, and on natural theology, with various quotations from their works.

It appears from this manuscript, that NEWTON was not only well acquainted with the opinions and reasonings of the ancients upon these subjects, but that he has done ample justice to their sagacity. This is a point which it is of some importance to have ascertained, as it has been asserted, particularly by M. DUTENS, in his "Recherches sur l'Origine des Découvertes attribuées aux Modernes," published in 1766, that the modern philosophers supposed the principle of universal gravitation to have been quite unknown to the ancients, and had therefore claimed the merit of the first discovery *. The authorities and quotations

* His expressions are, " C'est ici ou les modernes se flattent d'avoir un avantage, marqué, s'imaginant avoir les premiers découvert le principe de la gravitation universelle, qu'ils regardent comme une verité qui avoit été inconnue aux Anciens.

adduced by M. DUTENS to prove that the general principles of motion and gravitation were known to the ancients, are precisely the same as those contained in NEWTON's manuscript, a considerable part of which, I find, had been long before published, nearly verbatim, in the preface to the "Astronomiæ Physicæ et Geometricæ Elementa" of DAVID GREGORY *. The passage from LUCRETIUS, brought forward by M. DUTENS as a proof that the resistance of the medium through which they pass, was known to the ancients to be the cause of the difference in the velocity with which bodies fall, is quoted at full length by NEWTON after the following remarks:—"Et quamvis res leviores quæ aëris vel aquæ resistentiam difficilius vincunt in his fluidis descendant tardius, tamen in spatio vacuo ubi nulla est resistentia, atomos omnes tam graviores quam minus graves propter gravitatem sibi proportionalem æquali celeritate descendere, sic docet LUCRETIUS:" And the two remarkable lines,

> "Omnia quapropter debent per inane quietum,
> Æque ponderibus non æquis concita ferri,"

are written in large characters, with a view, evidently, to shew the importance which he attached to them.

This account of the opinions of the ancients occupies the greater part of the manuscript; but attached to it there are three very curious paragraphs. Two of these appear to have been the

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Il est cependant aisé de faire voir qu'ils n'ont fait que suivre les traces des ces anciens philosophes, en partant du même principe, et guidés par les mêmes raisonnemens:" p. 145.

* It is worthy of remark, that M. DELAMBRE, in his "Histoire de l'Astronomie au Dixhuitième Siecle," objects to these statements of DAVID GREGORY, as attributing more precise ideas on the subject of gravitation to the ancients than they possessed, not being aware that in so doing he was opposing not only the opinion of DAVID GREGORY, but that of NEWTON himself.

first draught of the general Scholium at the end of the edition of the Principia, published in 1713, and express the same theological opinion. It is remarkable, however, that it is only in the third edition, published in 1726, (the year before that in which NEWTON died), that the substance of the second of these paragraphs is found.

The first paragraph expresses nearly the same ideas as some sentences in the Scholium, commencing—" Deus summus est ens, æternum, infinitum, absolute perfectum:" The first part of it is as follows, and the expressions appear to me still more striking and sublime than those in the Scholium itself:—" Deum esse ens summe perfectum concedunt omnes. Entis autem summe perfecti Idea est ut sit substantia una, simplex, indivisibilis, viva et vivifica, ubique semper necessariò existens, summe intelligens omnia, libere volens bona, voluntate efficiens possibilia, effectibus nobilioribus similitudinem propriam quantum fieri potest communicans, omnia in se continens tanquam eorum principium et locus, omnia per præsentiam substantialem cernens et regens (sicut hominis pars cogitans sentit species rerum in cerebrum delatas, et illinc regit corpus proprium,) et cum rebus omnibus, secundum leges accuratas ut naturæ totius fundamentum et causa, constanter cooperans, nisi ubi aliter agere bonum est *."

The second paragraph expresses precisely the same idea as the sentences of the Scholium, in the edition of 1726, beginning, " A cæca necessitate metaphysica, quæ utique eadem est semper et ubique, nulla oritur rerum variatio;" and is as follows:—

* The remainder of this paragraph is as follows: " Nam liberrime agit quæ optima et ratione maxima consentanea sunt, et errore vel fato cæco adduci non potest ut aliter agat. Hæc est idea Entis summe perfecti, et conceptus durior Deitatem minime perficiet, sed suspectam potius reddet, aut forsan excludet e rerum natura."

"Quicquid necessariò existit illud semper et ubique existit, cùm eadem sit necessitatis lex in locis et temporibus universis. Et hinc omnis rerum diversitas, quæ in locis et temporibus diversis reperitur; ex necessitate cæca non fuit, sed a voluntate entis necessariò existentis originem duxit. Solum enim ens intelligens vi voluntatis suæ, secundum intellectuales rerum ideas, propter causas finales, agendo, varietatem rerum introducere potuit. Varietas autem in corporibus maxime reperitur, et corpora quæ in sensus incurrunt sunt Stellæ fixæ, Planetæ, Cometæ, Terra, et eorum partes."

The third paragraph relates to the same subject as the last paragraph of the Scholium, in which, as in his Optics, it is well known that Newton favours the hypothesis of a subtile and universally pervading Æther. But it is singular that it expresses upon this subject an opinion different from, and perhaps some may think sounder than, that which was afterwards published. This paragraph begins as follows:—"Cœlos et spatium universum aliqui materia fluida subtilissima implent, sed cujus existentia nec sensibus patet nec ullis argumentis convincitur, sed hypotheseos alicujus gratia præcario assumitur. Quinimo si et rationi fidendum sit et sensibus, materia illa e rerum natura exulabit;" and then proceeds to give reasons for this opinion, of the validity of which I do not pretend to judge *.

* The rest of this paragraph is as follows:—"Nam quomodo motus in pleno peragatur intelligi non potest; cùm partes materiæ, utcunque minutæ, si globulares sint, nunquam implebunt spatium solidum; sin angulares, propter omnimodum superficierum contactum firmius hærebunt inter se quàm lapides in acervo, et ordine semel turbato, non amplius congruent ad spatium solidum implendum. Porrò tam experimentis probavimus quàm rationibus mathematicis, quod corpus sphæricum densitatis cujuscunque in fluido ejusdem densitatis utcunque subtili progrediens, ex resistentia medii prius amittet semissem motus sui quam longitudinem diametri suæ descripserit. Et quod resistentia fluidi illius nec per subtilem partium divisionem, nec per motum partium inter se diminui possit, ut corpus longitudinem diametri prius describat quam amittat semissem motus."

This manuscript bears no date, but two circumstances enable me to state that it must have been written certainly eleven, and in all probability fifteen, years before the publication of the second edition of the Principia in 1713. *1st*, The edition of DAVID GREGORY's Elements of Astronomy, into which, as already stated, much of that portion of the manuscript which relates to the opinions of the ancients has been transferred almost verbatim, was published in the year 1702. *2d*, I find the whole of the manuscript fairly copied, in the handwriting of DAVID GREGORY, into the end of a manuscript book, containing his unpublished notes upon the Principia of NEWTON, and bearing a running date from 1687 to 1697. In 1702 Sir ISAAC NEWTON was not more than 60 years of age, and if, as appears almost certain, DAVID GREGORY received this manuscript from him between 1687 and 1697, he must have received it when NEWTON was between 45 and 55 years of age.

I do not know whether it is true, as stated by HUYGENS, "NEWTONUM incidisse in Phrenitin;" but I think every gentleman who examines this manuscript will be of opinion that he must have thoroughly recovered from his Phrenitis before he wrote either the commentary on the opinions of the ancients, or the sketch of his own theological and philosophical opinions which it contains.

Since the foregoing notice was read, some additional light has been thrown on the points to which it refers, by the publication of Sir ISAAC NEWTON's Correspondence with Mr LOCKE, in the Life of the latter by Lord KING. Two of NEWTON's letters to LOCKE, contained in this valuable work, might, at first sight, appear to favour the supposition of a temporary derangement in his intellect, more especially as they correspond in point of date with M. BIOT's "fatal epoch" of 1693; and as NEWTON himself states in one of them, that he had totally forgotten what he had writ-

ten on the subject of Locke's doctrine concerning innate ideas, scarcely three weeks before, in the other *.

But I believe it will be found, on closer examination, that these letters, unsupported by more direct evidence, will scarcely

* To enable the reader to form his own opinion, these two letters are here subjoined. They are both addressed to Mr Locke.

"Sir,

"Being of opinion that you endeavoured to embroil me with women and by other means, I was so affected with it, as that when one told me you were sickly and would not live, I answered, 'Twere better if you were dead. I desire you to forgive me this uncharitableness. For I am now satisfied that what you have done is just, and I beg your pardon for my having hard thoughts of you for it, and for representing that you struck at the root of morality in a principle you laid down in your book of Ideas, and designed to pursue in another book; and that I took you for a Hobbist. I beg your pardon also for saying or thinking that there was a design to sell me an office, or to embroil me. I am your most humble, and unfortunate Servant, Is. Newton."

"At the Bull, in Shoreditch,
London, Sept. 16. 1693."

In answer to these painful acknowledgments, Locke, in a letter written, as Mr Stewart justly remarks, "with the magnanimity of a philosopher, and with the good-humoured forbearance of a man of the world," requests Newton to point out the places in his book that gave occasion to his censure, in order that, by explaining himself better in a second edition, he may avoid being mistaken by others, or unawares doing the least prejudice to truth or virtue. Newton's reply is as follows:

"Sir,

"The last winter, by sleeping too often by my fire, I got an ill habit of sleeping; and a distemper which this summer has been epidemical, put me farther out of order, so that when I wrote to you, I had not slept an hour a night for a fortnight together, and for five nights together not a wink. I remember I wrote to you, but what I said of your book I remember not. If you please to send me a transcript of that passage, I will give you an account of it if I can. I am your most humble Servant, Is. Newton."

"Cambridge, *Oct.* 5. 1693."

warrant so harsh and uncharitable a conclusion. It appears from his own statement to LOCKE, that during the early part of the year 1693, NEWTON laboured under some bodily indisposition, attended with loss of sleep for a considerable time; and that his health had farther suffered by a disorder which had been epidemical during the summer of that year. The state of mental exhaustion, and probably of nervous irritability, brought on by this disorder of his health and want of rest, would, of itself, I conceive, go far under any circumstances to account for what it might otherwise appear difficult to explain in these letters. But when we take, at the same time, into consideration, that NEWTON, with all his boldness of conception and his extraordinary sagacity, was a man of a peculiarly timid and apprehensive character, and perhaps suspicious temper, or as LOCKE (certainly a very competent judge) expresses it, "a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground *;" I

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* These remarkable expressions occur in a very curious and interesting letter, also contained in Lord KING's Life of LOCKE. As it relates to the character of NEWTON, and appears to me to throw considerable light on the subject under discussion, I shall make no apology for inserting it here, especially as it seems to be but little known. It is a confidential letter from LOCKE to his friend and relation Mr King, afterwards Lord Chancellor; and it can scarcely be doubted that, had there been any truth in the supposition of NEWTON's previous derangement, he would have made some allusion to it on this occasion.

"DEAR COUSIN, *Oates, April* 30. 1703.

"I am puzzled in a little affair, and must beg your assistance for the clearing of it. Mr NEWTON, in autumn last, made me a visit here; I showed him my essay upon the Corinthians, with which he seemed very well pleased, but had not time to look it all over, but promised me if I would send it him, he would carefully peruse it, and send me his observations and opinion. I sent it him before Christmas, but hearing nothing from him, I, about a month or six weeks since, writ to him, as the inclosed tells you, with the remaining part of the story. When you have read it, and sealed it, I desire you to deliver at your convenience. He lives in German

think we can be at no loss to find a satisfactory explanation of these, as well as other letters he may have written during this period, without having recourse to the very improbable and gratuitous assumption of an idiopathic derangement of his intellect. Not only is there no direct evidence to support this assertion, but there is abundant proof that, during the alleged period of his insanity, he was employed, with all his characteristic vigour of mind and patient reach of thought, upon various abstruse and profound investigations. Among these may be mentioned the celebrated letters to Dr BENTLEY on the Existence of a Deity, which, though only published in 1756, were all written in 1692 and 1693.

Mr DUGALD STEWART, who had an opportunity of reading

Street: You must not go on a Wednesday, for that is his day for being at the Tower. The reason why I desire you to deliver it to him yourself is, that I would fain discover the reason of his so long silence. I have several reasons to think him truly my friend, but he is a nice man to deal with, and a little too apt to raise in himself suspicions where there is no ground; therefore, when you talk to him of my papers, and of his opinion of them, pray do it with all the tenderness in the world, and discover, if you can, why he kept them so long, and was so silent. But this you must do without asking why he did so, or discovering in the least that you are desirous to know. You will do well to acquaint him that you intend to see me at Whitsuntide, and shall be glad to bring a letter to me from him, or any thing else he will please to send; this perhaps may quicken him, and make him despatch these papers if he has not done it already. It may a little let you into the freer discourse with him, if you let him know that when you have been here with me, you have seen me busy on them (and the Romans too, if he mentions them, for I told him I was upon them when he was here,) and have had a sight of some part of what I was doing.

"Mr NEWTON is really a very valuable man, not only for his wonderful skill in mathematics, but in divinity too, and his great knowledge of the Scriptures, wherein I know few his equals. And therefore pray manage the whole matter so as not only to preserve me in his good opinion, but to increase me in it; and be sure to press him to nothing, but what he is forward in himself to do."

NEWTON'S letters to LOCKE, some years before they were published, saw nothing in them but "a humility and candour worthy of himself," and "an ingenuous and almost infantine simplicity." And, with respect to the opinion which NEWTON acknowledges he had entertained as to the tendency of LOCKE'S reasonings against innate ideas, Mr STEWART states, that he appears to have felt precisely in the same manner with Lord SHAFTESBURY, the author of the *Characteristics*, who, in his remarks on this subject, appears to Mr STEWART "to place the question about *innate ideas* upon the right and only philosophical footing; and to afford a key to all the confusion running through LOCKE'S argument against their existence *."

That LOCKE, who was intimately acquainted with NEWTON at the period of 1693, as well as for several years before and many afterwards, saw nothing in his conduct or correspondence to induce him to infer that he was at that time labouring under any degree of mental derangement, is evident from his requesting NEWTON, as a favour, to point out those passages in his essay which had appeared to him objectionable on account of their tendency, in order that he might correct them and explain himself better. Had there been any reasonable foundation for the charge of insanity, it is not likely that it would have escaped the penetration of so attentive and accurate an observer of the human intellect.

The publication of NEWTON'S correspondence with LOCKE, throws further light also on the period of his life at which he turned his attention particularly to theology and the study of the Scriptures. It appears clearly from NEWTON'S letters, that they

* Dissertation on the Progress of Metaphysical, Ethical, and Political Philosophy, Part II. p. 31.—Supplement to Encyclopædia Britannica, Vol. V. Part I.

corresponded on the subject of the Prophecies of Daniel, so early as the year 1691; and that the "Historical Account of Two Notable Corruptions of the Scriptures," which M. BIOT supposes NEWTON to have written about the year 1712 or 1713, when he was upwards of 70, was actually composed and transmitted to LOCKE (by whom it was forwarded to LE CLERC for the purpose of being translated into French) in the year 1690, prior to the alleged period of his insanity, and when he was only 48 years of age.

An Inquiry into the Geometrical Character of the Hour-Lines upon the Antique Sun-Dials. By T. S. Davies, Esq. F.R.S.Ed. F. R. A. S.

Il resterait à résoudre une problème plus générale, à trouver la courbe hectémoreale sur la sphère même, et sur un cadran quelconque.

Delambre, *Conn. des Temps, an.* 1820, p. 341.

(Read 21st Feb. 1831.*)*

The nature of these lines, though a subject of repeated investigation, has never been accurately determined. The loci of the points which divide the semi-diurnal arc into *n* equal parts, have almost invariably been considered as *great circles*, and their projections upon a plane, which are the dial-lines in question, in consequence viewed as straight lines. Clavius was the first to point out, and to prove, the error of this opinion; and Montucla (or rather Lalande) not only denied that they were circles at all, but affirmed they were curves of a very fantastical kind—" *tres bizarre.*" This latter opinion, which is partly true and partly false, seems rather to have been inconsiderately hazarded, than derived from any satisfactory course of reasoning. They are not great circles, it is true; but, as Delambre has replied, " il y a beaucoup d'exageration à donner une forme tres bizarre à des lignes qui, dans aucun des cadrons qu'on a tracées de cette manière, n'ont jamais pas s'ecarter sensiblement de la ligne droite *." Mr Cadell, the last author, except Delambre, who has attended to this subject, very clearly shews that the curve on the sphere is not a great circle; but he has not at-

* Conn. des Temps, pour l'an. 1820.—Mém. de l'Inst. tom. xiv. p. xxxi.

tempted any mathematical proof that it is not a less circle. His reasoning on this point, is not, even in form, any thing more than an appeal to experiment,—" *it does not look like a less circle, and therefore it is not one.*" " If," says he, " small circles, so placed, be drawn on the sphere, or projected on a plane, *it will be found* that their course deviates entirely from the course of the lines bounding the hectemoria. The hectemorial lines, *therefore*, do not coincide with small circles of the sphere, nor with conic sections on the central projection *." Delambre himself, indisputably the highest authority of his age on every thing relative to ancient astronomy, had always considered the hectemoria to be rectilinear, till the appearance of Mr Cadell's learned dissertation, when he discovered that his own equations indicated their variation from straight lines †." Still he has not examined what they really are: he has not shewn whether they be conic sections, or lines of a higher order, nor even distinguished whether they be algebraic or transcendental curves. Indeed, his equations ‡ were not calculated to shew the nature of the curves, however well they might, by a little address, have been adapted to the computation of the positions of isolated points in its course. It may be remarked of Mr Cadell's expressions ||, too, that they are equally incapable of furnishing the properties of the hectemoria as a class of curves; nor does it seem possible to extract from them any decisive proof whether they are the representations of conic sections or not. Indeed, had it been so, there is no doubt that Mr Cadell would have rendered them available for that purpose, instead of substituting the mechanical test of

* Edinburgh Transactions, vol. viii. p. 68.

† Conn. des Temps, *ubi sup.*

‡ Histoire d'Astron. Ancien. tom. ii. p. 475.

|| Edin. Trans. *ubi sup.* p. 65.

experiment for the mathematical syllogism, which the subject unquestionably both admitted and required.

With these facts in view, it appeared to me the most satisfactory, as in the end it proved to be the most simple, method of proceeding, to revert to first principles, and obtain, if possible, a general equation of these curves. As the result of my investigation sets the question at rest, by shewing the exact character of the curves; and as it has the advantage of being effected by principles which embrace all the circumstances of the inquiry, I beg to offer it to the Royal Society of Edinburgh, and venture to hope that it may be deemed worthy of a place in the Transactions of that learned body.

Bath, *February* 24. 1830.

THE EQUATIONS OF THE HECTEMORIAL CURVES UPON THE SURFACE OF THE SPHERE

I.

Referring the curve to great circle co-ordinates, originating at the elevated intersection of the meridian of the place with the equator, and estimated upon these circles; and moreover putting

I = inclination of the horizon and equator, or co-latitude of the place for which the dial is made;

D = declination of any one of the corresponding semi-diurnal arcs; and

L = longitude of a point in one of these curves whose declination is D;

then, by right angled spherical triangles,

$\sin^{-1}\tan D \cot I$ = ascensional difference; or complement of the semi-diurnal arc; and therefore

$$L = \frac{90 - \sin^{-1}\tan D \cot I}{n}, \text{ or}$$

$$\tan D = \tan I \cos nL \ldots\ldots\ldots\ldots\ldots\ldots (A_{D,L}).$$

Such is the general equation of the hectemoria upon the spherical surface, a few properties of which we proceed to examine, in order to furnish the means of ascertaining whether it be a circle or not.

II.

The curve is continuous, or the dial-lines in question form, generally, only a part of the curve whose equation we have just deduced. This will be apparent from the form in which it involves $\cos nL$, a quantity which will vary incessantly as L is made to vary. We shall trace it through one of its systems of changes.

Values of n L.		RESULTS.	INFERENCES.
= to	between		
0°	....	$\cos nL = 1$, $\tan D = \tan I$, and $D = I + \left\{\begin{matrix} 0^\circ \\ 180^\circ. \end{matrix}\right\}$	Hence the curve passes through the two intersections of the horizon with the meridian of the place.
......	0°, 90°,	$\cos nL < 1$, and decreasing continually as L increases.	The curve is concave towards the axis (equator).
90°	....	$\cos nL = 0$, $\tan D = 0$, and $D = \{0^\circ, 180^\circ,\}$	The curve cuts the axis in a point D (Fig. 1.) such that $AD = \frac{90}{n}$.
......	90°, 180°,	$\cos nL$ is — and increasing as nL increases.	The curve proceeds to the other side of LQ, and is concave towards its axis.
180°	....	$\cos nL = -1$, its greatest possible value on that side, $\tan D = -\tan I$; $D = -I + \left\{\begin{matrix} 0^\circ \\ 180^\circ. \end{matrix}\right\}$	The curve returns towards the axis.
......	180°, 270°,	$\cos nL$ is still negative, but diminishing in magnitude.	The curve bends down again towards the axis, being concave towards it, till
270°	....	$\cos nL = 0$, $\tan D = 0$, and $D = \{0^\circ, 180^\circ.\}$	The curve meets the equator in a a point R, such that $AR = \frac{270}{n}$.
......	270°, 360°,	$\cos nL$ becomes +, and increases on the positive side of the axis, and D increases also.	The curve is concave to the axis.
360°	....	$\cos nL = 1$, $\tan D = \tan I$, and $D = I + \left\{\begin{matrix} 0^\circ \\ 180^\circ. \end{matrix}\right\}$	The curve has arrived at a state similar to its original one.

By proceeding in the same manner, we shall find a repetition of the same results, and in the same order, till another circumference is added to the value of nL. Hence the several waves of curve are repeated at regular intervals upon the sphere, in unlimited succession.

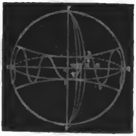
III.

Since, after 4AD of the abscissa is passed over, the curve returns to a position V, corresponding to that which we have taken as a starting point I, if 4AD be a submultiple of 360°, the curve, after one revolution of L, will fall upon that already traced out; that is, if AD be a submultiple of 90°, otherwise not. If AD be a submultiple of 2, 3, 4, ... quadrants, then, after so many revolutions of the value of L, the curves will return to their original positions, and retrace the same series of waves.

If 4AD, that is $\frac{4 \times 90^\circ}{n}$ be incommensurable with 360°, and all its multiples; or, in other words, if n be irrational, then no such reduplication of the curves can ever take place. In the hectemoria, properly so called, the value of n is, however, always rational.

It is plain, moreover, that all these branches are equal to one another, being similarly derived from trigonometrical functions, whose successive values at corresponding points are equal through all the mutations of sign and magnitude. If, however, a more detailed proof be required, it is easily effected as follows:

Let DW be taken (Fig. 1.) equal to DZ, but on the opposite side of D,



Then, $\cos n \,.\, AW = \cos(90 - n \,.\, WD) = \sin n \,.\, WD$
$\cos n \,.\, AZ = \cos(90 + n\, AZ) = -\sin n . AZ = -\sin n . WD.$

Hence the formula $(A_{D,L})$ becomes

$$\tan D = \tan I \sin n \,.\, WD \text{ for point W, and}$$
$$\tan D = -\tan I \sin n \,.\, WD \text{ for point Z:}$$

and the branches ID, DS at equal distances from the common extremity, are hence equal in all respects, but reversed in position.

IV.

The same great circle will be a tangent to both branches of the curve at D, but will be on different sides of it in the two hemispheres. For we have just seen, that the curve is composed of equal and similar branches, but reversed in position, and therefore we may at once infer, that any great circle through D will cut off equal portions from each side of it. Hence, when the circle YDY′ ceases to cut one branch, it ceases to cut the other, and it becomes simultaneously a tangent to each; and the curve has its point of contrary flexure at its intersection with the equator.

The same conclusion might, however, be readily obtained from the formula. For, consider Y a point in the hectemorion, and join DY by a great circle. Then, to find the spherical angle YDW, we have the sides DW, WY of the right-angled triangle WDY. Hence,

$$\sin WD = \cot WDY \tan WY, \text{ or}$$
$$\sin\left(\frac{90 - nL}{n}\right) = \cot WDY \tan D$$
$$= \cot WDY \tan I \cos nL, \text{ or}$$
$$\cot WDY = \sin\left(\frac{90 - nL}{n}\right) \cot I \sec nL.$$

If in this general expression of the value of WDY we make $nL = 90°$; then

$$\cot DWY = \frac{\sin 0° \cot I}{\cos 90°} = \frac{0}{0}.$$

To obtain, then, the ultimate value of cot WDY, take the differential co-efficients of the factors on the right hand side, instead of the factors themselves; we thus get

$$\cot \mathrm{WDY} = \cot \mathrm{I} \, . \, \frac{\dfrac{\cos\left(\dfrac{90 - n\mathrm{L}}{n}\right) \times - d\mathrm{L}}{d\mathrm{L}}}{\dfrac{- n \sin n\mathrm{L} \, . \, d\mathrm{L}}{d\mathrm{L}}} = \frac{1}{n} \cot \mathrm{I}, \text{ when } n\mathrm{L} = 90^\circ;$$

$$\text{or} \qquad \tan \mathrm{WDY} = n \tan \mathrm{I}.$$

Hence, also, the tangent of the angle which the curve makes with the axis at the point of intersection, is always n *times that of the inclination of the equator to the horizon.*

V.

We shall now discuss the equation $(\mathrm{A}_{\mathrm{D,L}})$ under another aspect, by taking the meridian P′AP as the axis, A being the origin as before. This investigation will be more conveniently conducted, by writing the equation in this form

$$\cos n\mathrm{L} = \tan \mathrm{D} \cot \mathrm{I} \ldots\ldots\ldots\ldots\ldots\ldots (\mathrm{B}_{\mathrm{D,L}}).$$

We must now recollect that the values of D are not intercepted upon great circles through E and Q, but upon less circles parallel to EQ. Our ordinates L, therefore, are to be estimated upon these parallels. It will readily appear, that this is but a polar equation of the curve.

Values of D.		RESULTS.	INFERENCES.
=	between		
I	....	$\cos nL = 1$, $L = \{0, 270\}$	The curve cuts the meridian at I.
......	I, 0°	tan D diminishes, and therefore cos nL tan I; and therefore again cos nL decreases, and nL is enlarged.	The curve expands on the positive side the abscissa.
0°	....	$\cos nL = 0$, and $L = \frac{90}{n}$.	This value of L, corresponding to $D = 0$, is the same as found in the former process.
......	0°, — I	cos nL is —, and hence the quantity is greater than 90°.	The curve continues to expand on the positive side of the abscissa L.
—I	....	$\tan D \cot I = -1$, $nL = 180°$, $L = \frac{180}{n}$.	The same result for the relation of D and L at the second intersection, as before obtained.
......	± I, to 180° — I,	$\cos nL \not> (\pm I)$, which is impossible.	Hence the curve has no point lying beyond the declination = I in either hemisphere.
	180° — I, 180° + I,	The same system of changes takes place on the other side of the globe, that is, below the horizon, the corresponding points of the two systems being diametrically opposed. Hence, if we conceive a diameter to move along one system, its opposite extremity will trace out the other system. This series is that of the nocturnal hectemoria, or the lines which divide the semi-nocturnal arcs into $n$ths. Or it is the system belonging to the antipodes of the former.	

If we put the equation $(B_{D,L})$ into the form

$$L = \frac{\cos^{-1} \tan D \cot I}{n},$$

it is very obvious, that, for any specific value of D, there will be as many values of L as there can be conceived circular revolutions; that is, an unlimited succession. If n be irrational, they will be all different from one another: if n be fractional or integral, they will recur in the same order,

after certain intervals corresponding thereto, and fall upon the original series, *ad infinitum*.

If n be a proper fraction, the quantity L must be greater than the semi-diurnal arc, and the curve will pass into the nocturnal portion of the sphere, or proceed beyond and rise up again on the diurnal portion. These positions may be fixed after the occurrence of several revolutions, depending, as is too obvious to need further specification, upon the value of the fraction n.

VI.

The question then, at issue, is decided. The hectemoria are not circles; for the circle has not one property in common with those we have shown to characterise this class of curves.

VII.

The *species* of a curve depends upon the relation amongst its constants; the *order* upon the relation among its variables. It often happens, however, that specific relations amongst the constants, affects also the *order* of a curve; and whenever it does so, it is by depriving it of its highest terms, or by destroying all those which do not contain a variable factor common to the whole. This circumstance takes place in the case before us; for though, generally, the curve upon the sphere is of a higher order than the circle, and consequently is a curve of double curvature, yet, in particular cases, it becomes a *great circle* of the sphere. It never, however, becomes a *less circle*. The general equation $(A_{D,L})$, and its modifications, determine these cases with great simplicity.

1. Take $$L = \frac{\cos^{-1} \tan D \cot I}{n}.$$

If, in this respect, $I = 90°$, then $\cot I = 0$, and

$$L = \frac{\cos^{-1} 0}{n} = \frac{90}{n}, \text{ for every point in the curve.}$$

Hence, these hectemoria are the equinoctial hour-lines of the sphere,

and I becomes the pole P. This agrees with fact; for the semi-diurnal arcs being all semicircles, the meridians, of course, n sect them all.

2. Take the equation in its original form, viz.

$$\tan D = \tan I \cos nL;$$

and let $I = 0$. Then $\tan D = 0$ for all values of L; which indicates that the hectemoria in question have all disappeared. There are, in truth, now no semi-diurnal nor semi-nocturnal arcs, and therefore no hectemoria, the equator being in the horizon.

3. In the same equation, put $n = 1$; then

$$\tan D = \tan I \cos L.$$

This is the equation of a great circle whose inclination to L is I; in short equation of the horizon itself.

4. As we increase n, our curves approach the meridian more and more nearly; and when n is become infinitely great, we get

$$L = \frac{\cos^{-1} \tan D \cot I}{\text{inf.}}.$$

Hence, so long as the numerator is finite, the value of L is

$$L = 0,$$

which is the equation of the meridian.

Now, for any finite number of revolutions, the numerator is finite; and when that number is infinite, the expression becomes indeterminate. Hence the meridian is the *only assignable locus* when n is infinite.

These cases of circular hectemoria have been already noticed, both by Mr Cadell and M. Delambre; and they are probably the only ones in which the curve becomes so simplified.

VIII.

The origin may be transposed to any point on the surface of the sphere, but the formulæ to which such a transfer gives rise, are generally too complicated to be of much use. Fortunately, however, the transformations of this kind, which our present object requires, are extremely simple, being only to points in the equator, and to the equatorial poles.

The origin is transposed to any point in the equator whose longitude is given ($\mp L'$), just in the same manner as in rectangular co ordinates, by simply annexing this quantity to L in the general equation ($A_{D,L}$). We thus obtain

$$\tan D = \tan I \cos n(L \pm L') \dots\dots\dots (A_{D,L \pm L'}).$$

Again, it may be transposed to the equatorial poles in the following manner. Let

$$D' = 90 - D; \text{ then}$$
$$\tan D = \cot D';$$

or, taking the reciprocals of these, we shall reduce to

$$\frac{1}{\tan D} = \frac{1}{\tan I} \cdot \frac{1}{\cos nL}, \text{ or}$$

$$\tan D' = \cot I \sec nL \dots\dots\dots\dots (A_{D',L}).$$

The expression ($A_{D',L}$) just obtained is in fact only a polar equation of the curve; and it is easy to see how it might have been primarily derived from first principles, and the other equations from it, in an order exactly the reverse of that which we have followed. The equation is, moreover, one of the most important of all in our inquiry.

This equation reckons its angles L from the meridian of the place, PAP′; but it might be referred to any other meridian whose longitude is $\pm L'$ as before. Then,

$$\tan D' = \cot I \sec n(L \pm L') \dots\dots\dots (A_{D',L \pm L}).$$

IX.

We have hitherto considered only the general equation, without any reference to the numerical values of n. These, for the several hour-lines, are as below:

$n =$	The corresponding Hour-Line.	Equation of Hour-Lines.
$\frac{6}{6} = 1$	VI. and VI. o'clock hour-lines.	$\tan D = \tan I \cos L$
$\frac{6}{5} = 1.2$	VII. and V.	$= \tan I \cos \frac{6L}{5}$
$\frac{6}{4} = 1.5$	VIII. and IV.	$= \tan I \cos \frac{3L}{2}$
$\frac{6}{3} = 2$	IX. and III.	$= \tan I \cos 2L$
$\frac{6}{2} = 3$	X. and II.	$= \tan I \cos 3L$
$\frac{6}{1} = 6$	XI. and I.	$= \tan I \cos 6L$
$\frac{6}{0} = \text{inf.}$	XII. and XII.	$= \tan I \cos \text{inf.}$

X.

We have seen, that whatever be the value of n, the hectemoria pass through the intersections of the meridian and equator. When, however, the great circle chords of the hectemoria are substituted for them, these chords belonging to hectemorial arcs whose extremities have all the same declination (the tropics, for instance), these chords do not all tend to the same point on the sphere. Neither do the tangents to the hectemoria, at their intersection with the equator, tend to the same point.

The second is a case of the first; viz. when the declination of the parallel through which the chords of the hectemoria pass is 0; yet the simpler method of finding the co-ordinates in that case, is to discuss it independently of the general property.

The general equations of the two circles are,

$$\tan D = \tan B_{,} \sin \frac{90 - n_{,} L}{n_{,}}$$

$$\tan D = \tan B_{,,} . \sin \frac{90 - n_{,,} L}{n_{,,}}$$

In which $B_{,}$ $B_{,,}$ are the angles at which these circles intersect the equator, and $n_{,}$ $n_{,,}$ are the values of n in the general equation of the hectemoria, adapted to the hour-lines in question.

Now, if D′ be the declination of the parallel, then

$$L' = \frac{\cos^{-1} \tan D' \cot I}{n_{,}}$$

$$L'' = \frac{\cos^{-1} \tan D' \cot I}{n_{,,}}$$

$$\text{and hence } \tan B_{,} = \tan D' \operatorname{cosec} \left\{ \frac{90 - \cos^{-1} \tan D' \cot I}{n_{,}} \right\}$$

$$\tan B_{,,} = \tan D' \operatorname{cosec} \left\{ \frac{90 - \cos^{-1} \tan D' \cot I}{n_{,,}} \right\}$$

Which substituted in the equations of the circles, the values of D then equated, and L found, will give the following result:

$$\tan L = \frac{c_{,} \sin \frac{90}{n_{,}} - c_{,,} \sin \frac{90}{n_{,,}}}{c_{,} \cos \frac{90}{n_{,}} - c_{,,} \cos \frac{90}{n_{,,}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(1)$$

where $c_{,}$ $c_{,,}$ are put instead of the above named cosecants.

In the case of the tangents, however, we may more simply take for the values of $B_{,}$, $B_{,,}$ those given in IV. We shall thus get

$$\tan L = \frac{n_{,} \sin \frac{90}{n_{,}} - n_{,,} \sin \frac{90}{n_{,,}}}{n_{,} \cos \frac{90}{n_{,}} - n_{,,} \cos \frac{90}{n_{,,}}} \quad \ldots\ldots\ldots\ldots\ldots\ldots(2)$$

These values of tan L are functions of $n_{,}$ and $n_{,,}$, which vary when they vary, and are permanent when they become permanent. Hence, clearly, the great circles intersect in points whose co-ordinates L, D are vari-

able; that is, they do not pass through the same point on the surface of the sphere. The hectemorial chords will not, therefore, pass through the same point on the dial itself.

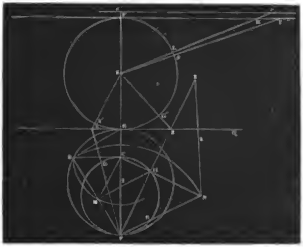
It is worthy of remark, too, that the *tangents* (Eq. 2.) intersect in current points whose ordinate L is independent of I. Hence the tangents to all the hour-circles which pass through the same points of the equator, will, taken two and two, and adapted to equal values of I, always intersect on the same meridian of the sphere.

XI.

We have now given the spherical equation, and examined a few of its properties, and shall proceed, in the next place, to give a gnomonic projection of these hectemoria upon any tangent plane.

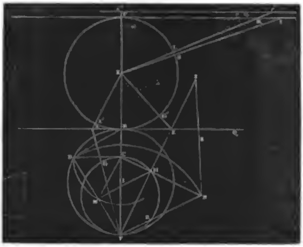
GNOMONIC PROJECTION OF THE HECTEMORIA UPON A GIVEN TANGENT PLANE.

General Preparation of the Dial.—Let CD be the radius of the sphere upon which the hectemorial curves are traced; C the point of con-



tact of the sphere and plane, BC, CP the tangent and cotangent of the

latitude of the point of contact, C of the sphere and plane. Draw BD, DP, and prolong BP, in which take BE = BD. With centre L and radius EB describe a circle BLF, and draw the indefinite tangents at B and F. With centre P and radius PD describe the circle DN, and upon CP as a diameter describe the circle CHP. Make the angle BEA equal to the longitude of the point of contact, and join AP. Take also E c = E c' = cot I = tangent of latitude of the place for which the dial is to be made; and draw the indefinite right lines $c\,r$, $c'r'$ parallel to the tangents FI, BQ.



Construction of Points in the Curves.—Take any value of L as AEZ, and join ZP, cutting the circle ICH in H. Repeat this angle (AEZ) n times, beginning at B *, and let BL be this multiple. Draw the secant EL, cutting $c'r'$ in s'; make FI = Es'; and join EI cutting the circle in θ.

Again, draw CH, cutting the circle DN in N; and make the angle PNS = BE θ. The intersection X of PZ, NS is a point in the curve.

* In the figure n is made = 2, being adapted to the IX and III o'clock hour-lines.

Demonstration.—By Horsley's Projections of the Sphere, B. iv. pr. 10. the angle APZ is the projection of the angle AEZ; and, as is obvious,

$$BEL = n \,.\, AEZ = nL.$$

$$\text{Hence, } ES' = \cot I \sec nL = FI = \tan BEs = \tan D'.$$

Again, draw CM parallel to PZ, and equal to CD, and join MH. Then CHP, HCM, and PHN are right angles; and we have

$$\begin{aligned} PD^2 &= DC^2 + CP^2 \\ &= DC^2 + CH^2 + HP^2 \\ &= CM^2 + CH^2 + HP^2 \\ &= MH^2 + HP^2. \end{aligned}$$

$$\text{But we have also } NH^2 + HP^2 = PN^2 = PD^2 = MH^2 + HP^2;$$

and hence, finally, $NH = HM$.

It follows, therefore, from Horsley's prop. 9. *ubi supra*, that the arc which measures the angle PNX is projected into PX; that is, the arc D′ is projected into PX. It has also been shewn that the angle AEZ is projected into APZ. Hence X is a point in the curve. Q. E. D.

XII.

The specific details of the construction are adapted to cases where the point of contact is not of a latitude either very high or very low. Very slight modifications, however, will adapt it to these cases also. The operations are intentionally rendered as simple as possible in reference to the intermediate latitudes, which could not have been done had we attempted in the same paragraph to render the description practically applicable to all cases. We proceed to consider these particular states of the problem.

1. *When the latitude of the point of contact is small.*—Let BC be the tangent of the latitude of the point of contact; CD, at right angles to it, the radius of the generating sphere; and AEZ any value of L as before. Draw DP′ at right angles to DB. In this case, the intersection of BC and DP′ may be considered as inaccessible, and the description of the circles DN and CHP as practically impossible. However, we are in possession of numerous processes for drawing lines which shall *tend* to the inaccessible intersection of two given lines; and by attending to the construction which was employed for facilitating the demonstration of the general method, we shall see at once that the circles are not essential to the construction of the problem itself, though, when they can be employed, they abbreviate the operation considerably. The substituted process, then, may be as follows.

Draw CH perpendicular to ZH (ZH tending to P); CM parallel to ZH, and equal to CD; and having joined MH, prolong CH till HN = HM. Draw NR′ tending to P, and make R′NX = BE θ. The intersection of HZ, NX gives X, a point the curve.

2. *When the latitude is 0°, or the plane touches the equator of the hectemorial sphere.*—The same course of reasoning leads to a very simple construction of this case also. For the point P having now become infinitely distant, the lines DP′, BC, ZH, NR′ are all parallel. The point H, moreover, coincides with Z, and C with B; and hence the point M falls in BC, and N in BZ. Hence,

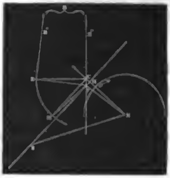
Take MB = radius of generating sphere, and find BE θ as before. Take Z, the point corresponding to A E Z, and join M Z. Make ZN = ZM, and draw NR′ at right angles to AZ;

and, finally, make the angle $R'NX = BE\theta$. The intersection of NX, ZH' will give X, as before, a point in the curve.

3. *When the latitude of the point of contact of the sphere and plane is considerable.*—By similar triangles,

$$BD\ (= BE) : BP :: DC : CP :: \text{rad} : \text{cosec}\ \lambda,$$

(λ denoting the latitude of the point of contact); and hence, if, on *any*



scale we take $B'E' : E'P' ::$ rad : cosec λ, we may proceed with the subsidiary operations for finding $B'E'\theta'$ as before, upon this new figure. The same relation will obviously subsist between $A'E'\theta'$ and its projection $A'P'Z'$, as between the corresponding angles upon the dial which we are constructing. We have, then, merely for any longitude $L = A'E'Z'$ to find $A'P'Z'$; to make $APZ = A'P'Z'$; and, finally, by the general process find PN, and make the angle $PNX = A'E'\theta'$. We thus get X.

4. *When* $\lambda = 90°$, *or the point of contact in the pole of the equator.*—In this case P, C, H coincide, and we have only to draw CN at right angles to CZ, and equal to the radius of the generating sphere; then making $CNX = B'E'\theta'$. We get X a point in the curve.

We might have supposed the coalescence of A and B in this last construction, since the pole may be deemed of any longitude.

It might further be observed, that, in the general construction, the circle BLF has been employed merely to give facility to the verbal description of the process, and to prevent the necessity of a separate construction to subserve the purposes of demonstration. In real practice it would, however, evidently be more convenient to keep it apart from the dial itself, as $A'B'\theta'$ was kept in (XII. 3.). The lines then employed upon the dial itself will be less numerous, and the process less complicated, than at first sight it may seem to be.

We have thus, by simple modifications of one uniform method of construction, shown how the hectemoria may be traced upon any plane whatever. So far, then, as the plane dial is concerned, *the problem proposed by* Delambre *is fully and completely resolved.* Dials, however, have been described upon other surfaces, which have been considered as hectemorial, and it may therefore, in continuance of the same system of investigation, be well to notice one or two of them.

XIII.

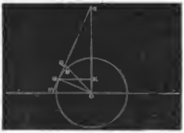
Cylindrical Hectemoria, in contact with the Equator.

Mr Cadell has given delineations of the hectemoria upon a cylinder touching the sphere at the equator (*Ed. Trans.* viii. Pl. 3. figs. 10—14). As the arc D of the equation ($A_{D,L}$) is projected into its tangent, this equation is a complete representation of that projection. We may hence construct those developed cylindrical hectemoria with great facility. It is, indeed, as will be shown farther on (see XXX.), *only a modification of the harmonic curve.*

XIV.

Conical Hectemoria.

In the second volume of STUART and REVETT's *Antiquities of Athens*, page 29, there is given a side and front delineation of a dial traced upon a conical surface, whose vertex is in the prolongation of the axis of the generating sphere. The lines traced upon this are, by Mr CADELL, considered to belong to the temporary system. Without anticipating what I have to say concerning this and some other specimens of ancient dials commonly referred to this class, I shall here investigate the equation and characters of a hectemorial conical dial, whose contact with the sphere is a circle parallel to the equator.



Let H be the vertex of the cone, G one of the points of contact of the cone and sphere. Let λ = latitude and L = longitude of G, reckoned from the meridian of the place; D = declination of one of the points of the spherical hectemoria corresponding to L, such that its value fulfils the equation

$$\tan D = \tan I \operatorname{cosec} L.$$

Let Q be the point upon the cone into which D is projected. The object, then, which we have in view, requires the value of the line HQ in terms of L, D, λ; or rather of L and λ, since D is a known function of L; we have,

$$\angle HOD = 90 - D$$
$$\angle HOG = 90 - \lambda'$$
$$\therefore \angle DOG = D - \lambda'.$$

Again, $HQ = HG - GQ = a(\tan HOG - \tan DOG)$

$$= a\left\{\cot\lambda - \frac{\tan D - \tan\lambda}{1 + \tan D \tan\lambda}\right\}, \text{ or, finally,}$$

$$v = \frac{a \operatorname{cosec}^2\lambda}{\cot\lambda + \tan I \cos nL} = \frac{a \operatorname{cosec}^2\lambda \cot I}{\cot\lambda \cot I + \cos nL} \ldots\ldots (C_{D,L})$$

XV.

We might have estimated the values of v from the intersection of the cone with the equator of the sphere, in directions tending to H. Thus, let F be the intersection of this arête of the cone with the equator; then,

$$v' = FG + GQ = a\left\{\tan\lambda + \frac{\tan D - \tan\lambda}{1 + \tan D \tan\lambda}\right\}$$

$$= a \cdot \frac{\tan D \sec^2\lambda}{1 + \tan\lambda \tan D} = a \cdot \frac{\sec\lambda \operatorname{cosec}\lambda \cos nL}{\cot I \cot\lambda + \cos nL} \ldots (C'_{D,L})$$

The chief difference between these two expressions, so far as utility is concerned, is, that the value of v has a constant numerator, while that of v' involves the variable factor $\cos nL$.

XVI.

If in the former of these equations $(C_{D,L})$ we put $\lambda = 0$, the result is,

$$v = \text{infinity};$$

indicating that the origin or vertex of the cone is infinitely remote.

But if we put $\lambda = 90°$, then we obtain

$$v = \frac{a \cot I}{\cos nL} = a \cot I \sec nL,$$

the same equation that we obtained for the hectemoria referred to the pole (VIII). We might here remark, that, in that equation, the arc D′ is projected upon a plane touching the sphere at the pole, into its tangent, and the angle L into an equal angle. That equation is, then, the equation of the projected hectemoria upon such a plane; and we are led to the same result by the equation which we have just obtained for the cone, the cone having merged into the tangent-plane when the latitude of its circle of con-

tact became 90°. We, however, shall have occasion to speak more amply on the subject farther on, and therefore dismiss it for the present.

If, on the other hand, we had used v', we should have had, when $\lambda = 0$,

$$v' = \tan \mathrm{I} \cos n\mathrm{L},$$

the equation of the hectemorial equatorial cylinder (XIII.), into which cylinder the cone is now transformed.

But if $\lambda = 90°$, then v' attains an infinite value, which shows that the intersection of the cone (that is, in this case, of the polar tangent plane) with the equator is become infinitely distant.

XVII.

When we develope a cone, the radius of the sector into which it is evolved is the arête of the cone itself. The radius of that sector is to the radius of the circle of contact (as HF : FO : : HG : GK : :) as cosec λ : 1. Hence all the values of L, reckoned from the beginning of the longitude, are *diminished* in the sector in the same ratio. Hence, to express the equation of the hectemoria upon such a conic surface when developed, we must take this change into account. Hence, if θ be the angle found between the radius sector v, and the origin of angular co-ordinates, the equations for v and v' will become,

$$v = \frac{a \operatorname{cosec}^2 \lambda \cot \mathrm{I}}{\cot \lambda \cot \mathrm{I} + \cos (n \,.\, \operatorname{cosec} \lambda \,.\, \theta)}$$

$$v' = \frac{a \operatorname{cosec} \lambda \sec \lambda \cos (n \,.\, \operatorname{cosec} \lambda \,.\, \theta)}{\cot \lambda \cot \mathrm{I} + \cos (n \,.\, \operatorname{cosec} \lambda \,.\, \theta)}$$

The latter, v', being always reckoned from the developed equator towards the centre of the sector.

XVIII.

These equations may be reduced wholly into factors, and thereby adapted to logarithms, though it will upon experiment prove of little advantage to do so, if facility of calculation be our object. In construction,

too, it is well understood by all who have attended at all to the practice of such operations, that, generally, those are the most easily constructed in which the fewest products or quotients appear. Yet as the method is easily derived; and, moreover, as in one particular case (and that the most important, the Choragic Dial at Athens being an alleged specimen) the formula possesses considerable simplicity, it is worth while to make the transformation.

Either $\cot\lambda \cot I$ is numerically greater than ± 1, or it is not.

Case I. $\cot\lambda \cot I > \pm 1$. Then put $\pi = \tan^{-1}\cos nL$, and $\mu = \tan^{-1}\cot I \cot\lambda$. Then we have

$$\cot\lambda \cot I + \cos nL = \tan\pi + \tan\mu = \frac{\sin\overline{\pi+\mu}}{\cos\pi\cos\mu}, \text{ and}$$

$$v = a \,.\, \cot I \operatorname{cosec}^2\lambda \cos\mu \cos\pi \operatorname{cosec}\overline{\pi+\mu}.$$

Case II. When $\cot\lambda \cot I = \pm 1$; that is, when the numerical values of λ and I are complementary.

$$\text{Here } +1 + \cos nL = 2\cos^2\frac{nL}{2}$$

$$-1 + \cos nL = 2\sin^2\frac{nL}{2}$$

And therefore the corresponding values of v are,

$$v = \tfrac{1}{2}a \sec\lambda \operatorname{cosec}\lambda \sec^2\frac{nL}{2}.\text{*}$$

$$v = -\tfrac{1}{2}a \sec\lambda \operatorname{cosec}\lambda \operatorname{cosec}^2\frac{nL}{2}.$$

Case III. When $\cot\lambda \cot I < \pm 1$; we have, putting $\nu = \cot^{-1}\cot\lambda \cot I$,

$$\cos\nu + \cos nL = 2\cos\tfrac{1}{2}(\nu + nL)\cos\tfrac{1}{2}(\nu - nL), \text{ and therefore,}$$
$$v = \tfrac{1}{2}a \cot I \operatorname{cosec}^2\lambda \sec\tfrac{1}{2}(\nu + nL)\sec\tfrac{1}{2}(\nu - nL).$$

---

* For, in this case, $\cot I = \frac{\sin\lambda}{\cos\lambda}$ $\therefore$ $\cot I \operatorname{cosec}^2\lambda = \frac{\sin\lambda}{\cos\lambda}\cdot\frac{1}{\sin\lambda^2} = \frac{1}{\sin\lambda\cos\lambda} = \sec\lambda\operatorname{cosec}\lambda.$

The first of the values of v in Case II. is that adapted to a hectemorial conical dial, such as the Choragic is stated to be. However, the discussion of the *applications* of these formulæ must be reserved till a future occasion, when we devote our attention particularly to that part of our subject. The values of v', too, in these several cases are so similarly obtained, that any details will be unnecessary, and we shall merely set them down.

Case 1. $v' = a \,.\, \operatorname{cosec}\lambda \sec\lambda \cos\mu \cos\pi \operatorname{cosec}(\pi + \mu) \cos nL.$

$$\text{Case 2.}\left\{\begin{aligned} v' &= \tfrac{1}{2}a \sec^2\lambda \sec^2\frac{nL}{2}. \\ v' &= \tfrac{1}{2}a \sec^2\lambda \operatorname{cosec}^2\frac{nL}{2}. \end{aligned}\right.$$

Case 3. $v' = \frac{1}{2}a \,.\, \operatorname{cosec}\lambda \sec\lambda \sec\frac{1}{2}(\nu + nL) \sec\frac{1}{2}(\nu - nL) \cos nL.$

XIX.

It is plain, that if for L we write $\operatorname{cosec}\lambda \,.\, \theta$, we shall have the equations of the hectemoria, when developed upon a plane: but it will, upon the whole, be more convenient, for the sake of general investigation, to put $n \operatorname{cosec}\lambda = m$, and we get the developed curve in an equation of the same form as that which represents the curve upon the conic surface itself. It is unnecessary to repeat them; and we proceed to ascertain their forms.

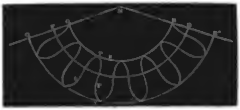
XX.

For this purpose we resume the first general form, viz. $(C_{D,L})$ found in XIV.

$$v = a \,.\, \frac{\operatorname{cosec}^2\lambda \cot I}{\cot\lambda \cot I + \cos mL},$$

merely putting m for $n \operatorname{cosec}\lambda$, to adapt it to the development aimed at.

There are several cases, according to the values and signs given to $\cot\lambda$ $\cot I$.



1. Let $\cot\lambda \cot I$ be greater than 1, and have the sign plus. It is plain that the denominator is always *positive* and always *finite*, and hence v must have values oscillating between *finite limits*.

If, on the contrary, $\cot\lambda \cot I$ be greater than 1, but marked minus, the denominator will always be *negative and finite*, and therefore oscillate between *finite limits*. The value of v, then, also possesses the same character, and differs from the other case only inasmuch as it is measured upon the radius sector in the reverse direction, or negative side of the pole of coordinates. The figures are in both cases exactly alike, then, in form, but reversed in position. The annexed sketch will give an idea of its character. The details of its course are easily laid down by enumerating the changes that result from the gradual change in the value of m L, and are too simple and obvious to need recapitulation, especially as they resemble the investigation contained in II. almost to identity.

2. Let $\cot\lambda \cot I = 1$; then when $m\mathrm{L} = (p+1) \times 90°$, the curves run out to infinity, and v becomes an assymptote. They then repeat the same system of changes through the next L, and so on without end. They have for the least value of v,

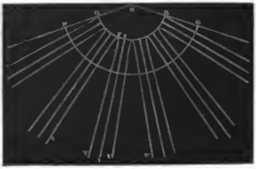
$$v = \frac{a}{2} \operatorname{cosec} \lambda \sec \lambda;$$

and where the curve crosses the equator, it is

$$v = a \operatorname{cosec} \lambda \sec \lambda.$$

If $\cot\lambda \cot I = -1$, the same curves and changes, but in a reverse order, present themselves. By comparing the sketch here given with Mr Cadell's projection on the equatorial tangent plane, it will be seen how much alike the curves are in the general form; but by comparing the equations of XVIII. 2. and that which will be given in XXV., a striking dif-

ference will be found between them; the one involves the secant of the variable angle θ, and the other involves the square of the secant of half the angle θ.

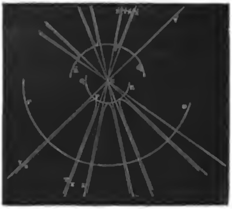


In the figures to this and the last case, FG is the projection of the equator upon the cone, QR the parallel of declination, which bounds the hectemoria on the hemisphere, which is touched by the cone. The opposite boundary in figure to Case 1. is Q′R′; but in the second case, it is infinitely distant.

3. Let $\cot\lambda \cot I \angle +1$. Then put $\cos\iota = \cot\lambda \cot I$.

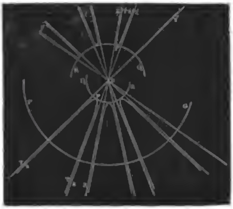
When $mL = \overline{180 - \iota}$, then the denominator vanishes, and the radius-vector r becomes infinite. The curve has at this value of L an asymptote.

During the onward motion of mL the denominator becomes negative, whilst the value of r diminishes in value till $mL = 180$, at which point the curve bends round, and forms an equal branch running off to infinity; which it attains when $mL = 180 + \iota$, and the curve has another asymptote. After this the denominator becomes positive, and continues so till $mL = 2\pi + \overline{180 - \iota}$; and it proceeds through the same system of changes as before.



Thus, if SPT be the branch corresponding to the values between $\overline{180 + \iota}$ and $180 - \iota$ as values of mL, then S′P′T′ will be that corresponding to the remaining portion of the circuit lying between $\overline{180 - \iota}$ and $180 + \iota$. The same alternation of opposite curves will succeed each other at regular intervals, as in the figure.

The parallel of declination bounding the hectemoria on the hemisphere of contact is QR, the equator is FG; and the parallel of declination bounding the hectemoria on the other hemisphere, is projected upon the *opposite* cone, and is developed into Q′R′. The negative branches of the developed curve, were, prior to development, situated upon the opposite cone also.



ON THE ANALYTICAL EQUATIONS OF THE HECTEMORIA.

We have seen that, on the sphere, the equations of the hectemoria involve a multiple cosine, and we may infer from the first principles of analytical geometry, that the same function must appear in all possible projections of their loci; with this addition, however, that the expression might be expected to assume, in general, a more complicated form. Still there is one process, and perhaps one only, by which an expression can be obtained from which the points of the projected curves can be computed with tolerable facility, more especially, if aided by one or two subsidiary tables. To proceed systematically, I shall commence by finding the equation of the hectemorial cone, concentric with the sphere, and referred to rectangular co-ordinates; and then proceed to determine its intersection with any plane whatever.

XXI.

The Equations of the Hectemorial Cone.—The spherical functions of latitude and longitude may be readily transformed into rectangular ones by

means of the common equations for the interchange of rectangular and polar co-ordinates. Thus, a being the radius of the hectemorial sphere, and putting $\tan I = i$,

$$\begin{aligned} z &= a \sin D, \\ y &= a \cos D \sin L, \\ x &= a \cos D \cos L, \end{aligned}$$

from which $\tan D = \dfrac{z}{\sqrt{x^2 + y^2}}$, and $\cos L = \dfrac{x}{\sqrt{x^2 + y^2}}$.

Then substituting these values in $(A_{D,L})$, we obtain

$$\frac{z}{\sqrt{x^2 + y^2}} = i \cos n \cos^{-1} \frac{x}{\sqrt{x^2 + y^2}} \ldots\ldots\ldots\ldots\ldots (A_{xyz})$$

which is the equation of the hectemorial cone.

XXII.

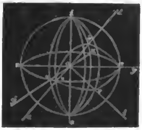
Our object being to find the intersection of this cone with a given arbitrary plane, the most obvious method of proceeding is to so transpose the origin and direction of the co-ordinate axes, that two of them shall lie in that plane, and the third be at right angles to it. The method of EULER, which is commonly employed for this purpose, and which is often found to be the most simple, both in its application and results, is, in the present instance, less convenient than the symmetrical formulæ of M. FRANÇAIS. I shall accordingly employ the latter method in the general transformation.

Let a, a', a'' be the cosines of the angles made by $\left.\begin{array}{l} x' \\ y' \\ z' \end{array}\right\}$ with x, y, z, respectively.

. . b, b', b'' . . . . y'

. . c, c', c'' . . . . z'

Then, draw OP perpendicular to the plane of projection, meeting the sphere in P, a point whose latitude is λ, and longitude l; and let this be taken as the axis of z'. Draw OP′ in the plane of the meridian zPz' perpendicular to OP, which take as the axis of x'. Lastly, draw OK perpendicular to POP′, for the axis of y'. The



* Journal de l'Ecole Polytechnique, cah. xiv. pp. 182—190.

values of the nine cosines above mentioned may then be formed into a tablet, as below.

Axes.	x	y	z
x'	$a = \cos XP' = \cos XL \cos LP'$ $= \cos l \sin \lambda$	$a' = \cos P'Y = \cos LP' \cos Ly$ $= \sin l \sin \lambda$	$a'' = \cos PZ$ $= -\cos \lambda$
y'	$b = \cos XK = \sin l$	$b' = \cos KY = \cos l$	$b'' = \cos KZ = 0$
z'	$c = \cos XP = \cos XL \cos LP$ $= \cos l \cos \lambda$	$c' = \cos Py = \cos PL \cos Ly$ $= \cos \lambda \sin l$	$c'' = \cos PZ$ $= \sin \lambda$

The several values of x, y, z, then, whilst the origin remains unaltered, will be

$$\begin{aligned} x &= x' \cos l \sin \lambda + y' \sin l + z' \cos l \cos \lambda. \\ y &= x' \sin l \sin \lambda - y' \cos l + z' \sin l \cos \lambda. \\ z &= x' \cos \lambda \qquad\qquad\qquad + z' \sin \lambda. \end{aligned}$$

If we substitute these values of x, y, z in the equation (A_{xyz}) we shall obtain the sheet of the hectemorial cone referred to the same origin, but new axes, x', y', z'; but we may refer them to any other origin by simply adding to the values of x, y, z the co-ordinates of that point. The object we have in view, taken in connexion with the form of the equation $(A\,x\,y\,z)$ requires that the addends to x and y should be both symmetrical and simple. There are four points which, under this aspect, offer themselves to our consideration, viz. the point of contact of the plane and sphere, and the three intersections of that plane with the axes of x, y, z.

The point of contact offers symmetrical results, its addends being

$$\begin{aligned} y'' &= a \cos \lambda \sin l, \\ x'' &= a \cos \lambda \cos l; \end{aligned}$$

but they are not in general the most simple. An exception to this happens when $\lambda = 0$, or the plane touches the equator, as will presently appear.

The intersection of x with the tangent plane, offers a result nearly the same in form as that given by EULER's formulæ of transformation; and, therefore, had this transformation been the most convenient, we might have adopted that formulæ at once. It is wanting, however, in symmetry, and is, besides, less simple than one yet to be noticed. The same objection, but in a stronger degree, exists against adopting the intersection of y, with the tangent plane for the origin.

There only remains, then, the intersection of z, and we proceed to give the results of taking this for the origin. The value of the addends are,

$$x'' = a \operatorname{cosec} \lambda,$$
$$y'' = 0,$$
$$z'' = 0.$$

Making these substitutions, and a few simple reductions, we arrive at length to

$$\frac{-x' \cos\lambda + z' \sin\lambda + a \operatorname{cosec}\lambda}{\left\{ (x' \sin\lambda + z' \cos\lambda)^2 + y'^2 \right\}^{\frac{1}{2}}} \times \cot \mathrm{I} =$$

$$= \cos n \cos^{-1} \frac{(x' \sin\lambda + z' \cos\lambda) \cos l + y' \sin l}{\left\{ x' \sin\lambda + z' \cos\lambda)^2 + y'^2 \right\}^{\frac{1}{2}}} \ldots (\mathrm{A}\, x' y' z'),$$

which is the equation of the hectemorial cone referred to its axes x' y' z', and originating at the intersection of z with the plane of projection.

XXIII.

To find the equation of the temporary hour-lines upon this plane, we have only to put $z' = 0$, and efface the accents from x', y'. Thus we have

$$\frac{-x \cos\lambda + a \operatorname{cosec.}\lambda}{\sqrt{x^2 \sin^2\lambda + y^2}} \cdot \cot \mathrm{I} = \cos n \cos^{-1} \frac{x \sin l \cos l + y \sin l}{\sqrt{x^2 \sin^2 \lambda + y^2}} \quad (\mathrm{A}\, xy).$$

Or changing rectangular into polar co-ordinates, r, θ, it becomes (dividing all by $\cos\theta$)

$$\frac{-r\cos\lambda + a\,\mathrm{cosec}\,\lambda\sec\theta}{r\sqrt{\sin^2\lambda + \tan^2\theta}}\cdot\cot\mathrm{I} = \cos n\cos^{-1}\frac{\sin\lambda\cos l + \sin l\tan\theta}{\sqrt{\sin^2\lambda + \tan^2\theta}}$$

or separating the variables,

$$r = \frac{a\sec\lambda\,\mathrm{cosec}\,\lambda\sec\theta}{\sqrt{\sin^2\lambda + \tan^2\theta}\cdot\sec\lambda\tan\mathrm{I}\cos n\cos^{-1}\dfrac{(\sin\lambda\cot l + \tan\theta)\sin l}{\sqrt{\sin^2\lambda + \tan^2\theta}} + 1}\ldots(\mathrm{A}_{r\theta}).$$

This expression admits of some simplification. For with respect to the radical which is involved doubly in the denominator, we may write it thus:—

$$\sin^2\lambda + \tan^2\theta = \sin^2\lambda + \sec^2\theta - 1 = \sec^2\theta - \cos^2\lambda = (\sec\theta + \cos\lambda)(\sec\theta - \cos\lambda) = \varrho^2,$$

which is adapted to logarithms; and λ is the same for the whole dial.

Again, for the numerator of the expression under the function $\cos^{-1}$, we may put

$$\nu = \tan^{-1}\sin\lambda\cot l.$$

Then we have

$$(\tan\nu + \tan\theta)\sin l = \sin\overline{\nu + \theta}\sec\nu\sec\theta\sin l,$$

which is adapted to logarithms; and ν and $\sec\nu\sin l$, are constant for the same dial.

Further, $\sec\lambda\tan\mathrm{I}$, and $\sec\lambda\,\mathrm{cosec}\,\lambda$, are constant for the same dial.

Lastly, if we make

$$\cos^{-1}\varrho\sec\lambda\tan\mathrm{I}\cos n\cos^{-1}\frac{\sin\overline{\nu + \theta}\sec\nu\sec\theta\sin l}{\varrho} = \mathrm{K},$$

we shall obtain for the equation of the hectemorial curves,

$$r = \tfrac{1}{2}a.\sec\lambda\,\mathrm{cosec}\,\lambda\sec\theta\sec^2\frac{\mathrm{K}}{2}.$$

Or, exhibiting the formula at one view in *a calculable state*, it will be,

$$r = \frac{a\sec\lambda\,\mathrm{cosec}\,\lambda\sec\theta}{\left\{(\sec\theta + \cos\lambda)(\sec\theta - \cos\lambda)\right\}^{\frac{1}{2}}\sec\lambda\tan\mathrm{I}\cos n\cos^{-1}\dfrac{\sin\overline{\nu + \theta}\sec\theta\sec\nu\sin l}{\{(\sec\theta + \cos\lambda)(\sec\theta - \cos\lambda)\}^{\frac{1}{2}}} + 1}\ldots\left(\mathrm{A}'_{r\theta}\right)$$

XXIV.

By giving to the quantities λ, l, n, various values, to adapt them to different dials, and to the several hour-lines upon each, we shall be able to determine for θ a corresponding value of r, and thus assign the assemblage of points which constitute the several hectemoria. Though it is not my intention in the present paper to enter upon the discussion of particular dials, yet it seems necessary to enter a little into discussion of the forms assumed by our equations when particular values are given to λ and l. It is necessary, both in illustration of the formula, and in order to divide the whole investigation into parts as nearly equal as is practicable.

1. Let $\lambda = 0°$; then for all values of l we shall have $\sec \lambda = 1$; $\operatorname{cosec} \lambda =$ infinity; and

$$r = \frac{a \sec. \theta \sec 0° \operatorname{cosec} 0°}{\tan \theta \tan I \sec 0° \cos n \cos^{-1} \sin l + 1} = \text{infinity}.$$

This shews that the origin is infinitely remote, as from other simple considerations we know that it should be. The method to be employed in this case will be presently explained.

2. Let $l = 0°$, and $l = 180°$, whilst λ is arbitrary. Then $\sin l = 0$; $\cos l = \pm 1$; and

$$r = \frac{a \sec \lambda \operatorname{cosec} \lambda \sec \theta}{\epsilon \sec \lambda \tan I \cos n \cos^{-1} \dfrac{\pm \sin \lambda}{\epsilon} + 1}.$$

This is the equation of all *north* and *south* dials, whatever be their inclination to the horizon.

3. If in (2) we put $\lambda = I$, we shall obtain the equation of the vertical *south* dial, such as the ΝΟΤΟΣ* of ANDRONICUS CYRRHESTES, figured in plate x. vol. i. of STUART and REVETT's Antiquities of Athens.

* It would have been more conformable to the common usage to have designated these dials by names diametrically opposite upon the compass; but if, in all cases, we

4. If we take $\lambda = \overline{90 + 90 - \mathrm{I}} = 180 - \mathrm{I}$, the dial will be *horizontal.* Of this kind, I believe, there is no specimen remaining in any collection.

5. If $\lambda = 270° - \mathrm{I}$, the dial is a *north vertical one*, such as the ΒΟΡΕΑΣ of Andronicus, a sketch of which is given in plate xi. vol. i. of Stuart.

6. For the four intermediary dials of Andronicus, we may readily find the values of l and λ, which correspond to each of them. For by right-angled spherical triangles we readily obtain

Formulæ.	Dials.	λ, l, for Athens (value of).
$\cot l = \cos. \mathrm{I}.$ $\sin \lambda = \sqrt{\frac{1}{2}} . \sin \mathrm{I}.$	S. E. or ΕΥΡΟΣ.	$l = +58° 40' 7''$ $\lambda = +34\ 7\ 27$
Ibid.	S. W. or ΛΙΨ.	$l = -58\ 40\ 7$ $\lambda = +34\ 7\ 27$
Ibid.	N. E. or ΚΑΙΚΙΑΣ.	$l = +148\ 40\ 7$ $\lambda = -34\ 7\ 27$
Ibid.	N. W. or ΣΚΙΡΩΝ.	$l = -148\ 40\ 7$ $\lambda = -34\ 7\ 27$

7. In like manner, by the solution of a right angled spherical triangle,

imagine the dial plane, after the hectemoria are traced, to be revolved upon its intersection with the horizon through 180°, and the gnomon to be a continuation of the radius of the sphere and equal thereto, the extremity of it will be the gnomonic point to the dial as it is now placed. The reasons for this departure from common usage cannot be conveniently explained here: nor is it of importance that it should be, a bare indication of the fact being sufficient to prevent misapprehension.

we obtain the values of λ, l for the four vertical dials of PHÆDRUS in Lord ELGIN's Collection *. These are,

Dial (Azimuth).	Value of λ.	Value of l.
46° 11′ 50″ East	+ 32° 58′ 35″	+ 60° 8′ 30
.... West	+ 32 58 35	— 60 8 30
39° 10′ 24″ East	+ 38 3 28	+ 53 10 21
.... West	+ 38 3 28	— 53 10 21

XXV.

We come now to the case which Mr CADELL has so well illustrated by his accurate drawings †. This is, in every point of view, the simplest case that can be taken, and was well adapted to a first inquiry into the general character of the curves. Mr CADELL has also given an equation of the projected curves upon the polar tangent plane, and deduced from it some general inferences, which are abundantly justified by our general investigation.

In this case, the expression is reduced to

$$r = \frac{a \sec \theta \operatorname{cosec} 90° \sec 90°}{\sec 90° \sec \theta \tan \mathrm{I} \cos n \cos^{-1} \cos (l \backsim \theta) + 1}$$
$$= a \cot \mathrm{I} \sec n \, (l \backsim \theta).$$

Keeping in mind that the general equation was referred to the axis of x' as the origin of θ, it will be at once obvious, that if we write θ' instead of $(l \backsim \theta)$ we shall refer the equation to the meridian of the place as the origin of polar co-ordinates, and the expression will be transformed into

$$r = a \cot \mathrm{I} \sec n \theta' \; . \; . \; . \; . \; . \; (\mathrm{A}_{r\theta'})$$

* Erroneously stated in the "Memoranda of Lord ELGIN's pursuits in Greece," to be from the Temple of Bacchus. Mr KINNAIRD saw the dial in the Temple of Bacchus in its original position in 1818. See his Notes to the new edition of STUART's Athens. In the present arrangement of the Elgin Gallery, in the British Museum, this dial stands opposite the stairs, in the angle of the room.

† Edinburgh Transactions, vol. viii. p. 79, and the corresponding plates.

The same result has, however, been already obtained (XV.) by means of the central projection of the general equation upon the polar tangent plane. For it is obvious that tan D' of that equation is the r of this; and that L of that is t' of the present. The two equations

$$\tan D' = \cot I \sec n L$$
$$r = \cot I \sec n t'$$

evidently differ then in nothing but the notation.

But, independently of the general equation on the sphere, or its projection, the equation of these curves upon the polar tangent plane might have been readily found. For, let A be the pole, D the intersection of the meridian with the horizon, and BGC the projection of any semidiurnal arc. Let also E be one of the hectemorial points in that circle. Also let a = radius of the sphere, and r = AE.

$$AD = \cot I$$

$$\frac{a \cot I}{r} = \cos GAC, \text{ or } GAC = \cos^{-1} \frac{a \cot I}{r}.$$

$$\text{Hence } \widehat{GE} = t' = \frac{1}{n} \cos^{-1} \frac{a \cot I}{r}; \text{ or, finally, by reduction,}$$

$$r = a \cot I \sec n t'.$$

XXVI.

The equation employed by Mr Cadell certainly shews that the locus is not, generally, a straight line; and, therefore, that on the sphere the hectemoria are not great circles: but he does not attempt to prove by means of it that the locus is not a conic section, and that therefore the curve on the sphere cannot *possibly* be a less circle. I say "possibly," for we should not be justified in the inference, that because the projection was a line of the second order, the curve itself was, *therefore*, a less circle,—an oversight that has been inadvertently made by more than one respectable geometer. The curve of penetration of a sphere with a concentric cone of

the second order is commonly of double curvature, and its orthographic projection, like that of the intersections of surfaces of the second order in general, is of the fourth degree.

But to return. If Mr CADELL's equation was less simple and perspicuous than it might have been rendered, it was owing to the accident of adopting rectangular instead of polar co-ordinates, and of taking the vertex of the curve instead of the centre of projection for the origin of the co-ordinates. Yet one advantage has resulted from it, that he was driven to pursue his investigations graphically, and thereby exhibit to the eye the general figure of the curve, as perfectly as any equation could exhibit it to the understanding. Indeed, they exhibit the course of the curve as clearly as if it had been actually traced on the sphere itself. Indisputably, then, he has the honour of being the first who clearly understood the figure of these curves, as well as the first to illustrate their general character by actual delineation. Had he been equally happy in his analytical investigations, he would have rendered this dissertation a work of supererogation; and it is more than probable that the present investigation would never have been undertaken. He would then, also, whilst he carried conviction to the mind of DELAMBRE, (which had already been made up on the contrary side of the question), have produced a change in the details of the chapter on the "Analemma" in the History of Ancient Astronomy of that illustrious geometer.

XXVII.

The equation ($A_{r\theta}$) does not, as appears from (XXV. 1.), include the cases (at least it does not express the relation between the co-ordinates in finite terms) where the point of contact is in the equator of the sphere; such, for instance, as the *east and west vertical dials*, or the ΖΕΦΥΡΟΣ ΑΠΗΛΙΩΤΕΣ of "the Tower of Winds" at Athens *. The process is nevertheless very simple.

* STUART, vol. i. pl. 10.

Let them be referred to the point of contact as origin, that is, the *eastern* dial to the east point of the horizon, and the western to the west. Then $\lambda = 0$, $l = \pm 90°$; the addend to y is $\pm a$, and x and z, each 0. Hence the values of x, y, z, in (XXII.) become,

$$x = \pm y'$$
$$y = \pm z - a$$
$$z = -x'$$

and the expression is reduced to

$$x' = i\sqrt{y'^2 + a^2} \cos n \cos^{-1} \frac{\pm y'}{\sqrt{y'^2 + a^2}}$$

which is the equation of the hectemorial curves upon the dials touching the sphere at the east and west points of the horizon, and referred to the rectangular axes x', y':

But since, from the method of transformation that we have employed, the axis of y' is the intersection of the dial with the equatorial plane, it readily occurs to us that the better way will be to interchange the x' and y' at the same time that we cancel the accents. Hence

$$y = \sqrt{x^2 + a^2} \tan \mathrm{I} \cos n \cos^{-1} \frac{\pm x}{\sqrt{x^2 + a^2}} \quad \ldots\ldots \quad (\mathrm{A}_{e,w})$$

A further transformation will greatly facilitate the practical calculation of these dials. Let $\varphi = \tan^{-1} x$, (rad. sphere $= a$); then

$$\sqrt{x^2 + a^2} = \sec \varphi$$
$$y = a \tan \mathrm{I} \sec \varphi \cos n \cos^{-1} \sin \varphi$$
$$= a \tan \mathrm{I} \sec \varphi \cos n\, (90° - \varphi), \text{ or, restoring } \tan^{-1} x,$$
$$= a \tan \mathrm{I} \sec \tan^{-1} x \cos n\, (90° - \tan^{-1} x) \quad \ldots\ldots \quad (\mathrm{A}_{e',w'})$$

Using Hutton's tables, where the natural and logarithmic functions of the angle φ stand at the same opening, we find the value of y with a single opening of the book, so long as we suppose φ to vary from step to step by some exact number of minutes—a degree of precision which may be deemed greatly more than sufficient.

XXVIII.

In the same manner, but with a rather less simple result, we might obtain the equation of the hectemoria upon any plane parallel to the earth's axis. We might also, by the formula of EULER, attain the same object; and it will be found upon trial to be in exactly the same form.

For, in this case, our x and y coincide with his y and x respectively. Denoting, then, by t the trace of the equatorial upon the dial plane, and taking the other quantities in the usual manner, we obtain by EULER,

$$x, x' = 90 - l, \text{ and } x\,y, x'\,y' = 90^\circ$$

$$\text{hence } x = x' \sin l + a \cos l$$

$$y = - x' \cos l + a \sin l$$

$$z = y'$$

$$\therefore x^2 + y^2 = (a + x')^2 \text{ and the hectemorial equation becomes,}$$

$$y = i \sqrt{x^2 + a^2} \cos n \cos^{-1} \frac{x' \sin l + a \cos l}{\sqrt{x^2 + a^2}} .$$

Or, if we put $a \cot l = b$, it is

$$y = i \sqrt{x^2 + a^2} \cos n \cos^{-1} \frac{(x + b) \sin l}{\sqrt{x^2 + a^2}} .$$

$$= a \tan \mathrm{I} \sec \tan^{-1} x \cos n \cos^{-1} \left\{ \frac{x + b}{a} \sin l \cos \tan^{-1} x \right\} \; \ldots\ldots \; (\mathrm{A_E})$$

Making the interchange already noticed, and dropping the accents from x', y', we shall obtain the result from the method of M. FRANÇAIS. It must be recollected that l is the longitude of the point of contact of dial with the hectemorial equator.

XXIX.

We have considered the east and west dials, and obtained for them a simple expression in XXVII., and we can in the same manner obtain an

expression for the "polar dial *,"—either from the same special method there employed, or as a case of XXVIII. The latter is the readiest.

Here, then, $l = o$, (or $l = 180$ for the other intersection); and therefore $x' \sin l = o$, $a \cos l = a$;

$$y' = i\sqrt{x'^2 + a^2} \cos n \cos^{-1} \frac{a}{\sqrt{x'^2 + a^2}}$$

But if $\tan^{-1} x' = \varphi$, there will result $\frac{a}{\sqrt{x'^2 + a^2}} = \cos\varphi$; and, dropping accents,

$$\begin{aligned} y &= a \tan \mathrm{I} \sec \varphi \cos n \cos^{-1} \cos \varphi \\ &= a \tan \mathrm{I} \sec \varphi \cos n \varphi \\ &= a \tan \mathrm{I} \sec \tan^{-1} x \cos n \tan^{-1} x. \end{aligned}$$

The first branches of these hectemoria are traced by Mr Cadell †, which, though less accurate than his other drawings, are yet sufficient to give a general notion of their character. They seem to be sketched without much instrumental assistance, or reduced by the eye from larger projections.

XXX.

It has already been stated, that the hectemoria being traced upon a sphere, and projected upon a cylinder touching it at the equator, is but a modification of the harmonic curve ‡. The equation of the curve upon the developed cylinder (the axis of x being the unrolled equator, and y the corresponding value of tan D), is reduced to

$y = i \cos n x$; or when referred to radius a,

$$y = ia \cos n \left(\frac{x}{a}\right)$$

Put $\frac{a}{n} = a'$ or $n a' = a$; then $y = n a'. i. \cos\left(\frac{n x}{n a'}\right)$

$$= i. n a. \cos x.$$

* That is, a dial which touches the hectemorial sphere at the intersection of the equator and meridian of the place for which it is made.

† Plate II. fig. 3. of the Memoir already referred to.

‡ Art. XIII.

This is the equation of the harmonic curve, whose modulus is $n\,i$, and the radius of its generating circle $= a'$. *The hectemoria, therefore, when projected upon the equatorial cylinder, and that cylinder developed, is the* HARMONIC CURVE, *the modulus of which is* n i, *and radius of its generating circle* = a′.

Again, the harmonic curve is the development of an elliptical section of a right cylinder, and hence we are put into possession of a *mechanical* method of tracing the curves. Thus:

A right cylinder to radius $\frac{a}{n}$ or a' being cut by a plane which makes with the axis an angle $= \tan^{-1} n \tan \mathrm{I}$, being developed indefinitely, will give the hectemoria in question, *in plano*. Rolling this plane hectemorial series upon a cylinder to radius a, we obtain the cylindrical hectemoria. By cutting away the upper or the lower part of this cylinder by lines from the centre of the equatorial section, would give a ruler upon which a line moving about that centre would trace the plane hectemoria upon a plane anyhow situated with respect to the ruler. The mechanical contrivances requisite to effect this are not numerous nor complicated; but great care is essential in the actual structure of such an apparatus to ensure a desirable degree of accuracy. These subjects, however, do not properly belong to the present place.

XXXI.

The decision of this long disputed question in the history of Astronomy is, I trust, now effected. We see what the lines really are, and are well assured that their deviation from these chords within the range required by the ancient astronomer could not have been discovered by any methods within his reach; and that, as he had no means of tracing their prolongations, he could not arrive at any true notion of their character. Even had he been in possession of tolerably accurate methods of measuring time, he would rather have been disposed to attribute any slight discrepancies between his clock and his dial to errors of observation, than to any deviation of the true hectemoria from a rectilinear course. It was from considerations altogether

tions of the angles, the functions of which enter into the formula just given. The value of L in that case would be

$$L = \frac{1}{n} \sin^{-1} \left\{ \frac{1}{n} \cos 109° \, 49' \, 50'' \operatorname{cosec} \frac{19° \, 49' \, 50''}{n} \right\}$$

and the only difference is, that the errors (the same in value) are affected by the contrary sign.

It has been remarked that the results given by the formula above are *always greater* than the error arising from the substitution of great circles for the true hectemoria. A few simple considerations will establish this.

Let the plane of that great circle on the sphere which joins the equinoctial and tropical hectemorial points be produced to cut the equatorial cylinder. This cylinder being again developed, the elliptic section thus made becomes a new harmonic curve. We have to prove that this new harmonic curve passes between the former and the chord of its developed intertropical segment.

The harmonic curve is always concave to its axis, and hence all the chords, which lie wholly on one side of the axis, lie wholly within the curve. Now, both these harmonic curves are referred to the same axis, and cut one another, and hence the common chord must fall within them both; or, which is the same thing, they both lie on the same side of the common chord. Evidently, then, that which has the least curvature must fall between the other and the common chord. But the extreme values of y being the same, that curve will have the least curvature whose range of abscissa is the greater, or, which is the same thing, whose generating circumference, and consequently generating radius, is the greater. Hence the new harmonic curve falls between the other curve and the common chord; and, therefore, if we restore the developed cylindrical surface to its former equatorial position, the great circle which forms the spherical chord of the intertropical hectemoria will fall *between* that in hectemoria and the line which formed the rectilinear chord of the harmonic curve. The abscissal distance between the great circle chord and the true hectemoria, for any specified de-

ascension is to be calculated. Draw CH parallel to AB, cutting the line BD in K, and draw the ordinates CE, KF.

Then $FC = n\,i \cos AF$ by the equation of the developed hectemoria
$EK = (AB - AF + FE) \times \tan B$ from the line BD,
but $FC = EK \therefore n\,i \cos AF = (AB - AF + FE) \times \tan B$,

$$\text{or, } EF\ (= KC) = \frac{n\,i \cos AF + AF \tan B - AB \tan B}{\tan B}.$$

Putting its differential equal to *o*, we have

$$\sin AF = \frac{\tan B}{n\,i} = \frac{\tan B}{n.\tan I}.$$

$$= \frac{1}{n} \cos n\,L' \operatorname{cosec} \frac{90 - n\,L'}{n}$$

where $n\,L'$ = semidiurnal arc, at the declination D', derived from the equation

$$\cos n\,L = \cot I \tan D'.$$

For the obliquity of the ecliptic as determined by Hipparchus, and the ancient estimate of the latitude of Athens, viz. 23° 51′ and 37° 30′, we find

$$n\,L' = 70° \, 10' \, 10''$$

$$\sin L = \sin AF = \frac{1}{n} \cos 70° \, 10' \, 10'' \operatorname{cosec} \frac{90° \, 49' \, 50''}{n}$$

But the radius of the cylinder from which this curve was developed was $\frac{a}{n}$, and the arc AF, therefore, when transposed to a cylinder whose radius is a, will measure but $\frac{1}{n}$th of the same angular magnitude. We shall hence obtain the value of L where the deviation is a maximum, viz.

$$L = \frac{1}{n} \sin^{-1} \left\{ \frac{1}{n} \cos 70° \, 10' \, 10'' \operatorname{cosec} \frac{19° \, 49' \, 50''}{n} \right\}$$

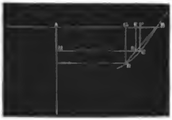
From the perfect equality of the two branches of the curve, we readily infer that the same amount of error will be found at the corresponding declination in the other hemisphere. We may also infer it from the muta-

different from any that were entertained by the Egyptian, Babylonian, or Grecian philosophers, that we have discovered the true character of the lines; and it has been mentioned by Mr Cadell, and calculated by Delambre, how little these lines in the intertropical regions deviate from their chords, in right ascension. This, however, is more distinctly brought under our notice by the fact, that the developed hectemoria is identical with a series of harmonic curves, the characters of which have been often examined, and are well understood. It is not easy, nevertheless, to ascertain by direct investigation the *greatest error* that can result from the adoption of the hectemorial chord for the curve itself, between certain assigned limits of declination D′. Treated by the usual methods of maxima and minima we are led to an equation of the form

$$E.\ \sin n\dot{L} = \cos\frac{90 - nL}{n}.$$

This equation, it is at first sight obvious, can be solved only by a tedious approximation, the convergency of which is not only slow, but the direction and amount of the correction at each step, very difficult to assign. Delambre, therefore, calculated a few of the errors which belonged to different declinations and latitudes, and for each value of n which entered into the expression for the twelve hour-lines. These he found to be very minute.

It is still possible, by means of the direct calculus, to find a quantity for each hour-line in a dial adapted to any latitude which shall differ but little from the maximum error, and that quantity at the same time shall be always greater than the maximum error. This is founded on the same property that the developed hectemoria becomes the Taylorean Curve.



Let A be the origin, AB the axis of the harmonic curve, find the angle B which corresponds to the value D′ of the declination and for the hour-line in question. Let C be the point in the curve for which the error in right

clination less than D′, is always less than the abscissal distance of points in the hectemoria, and their rectilineal chords at the same declination.

These conclusions might also have been readily established by means of the differential calculus; but that is perhaps, at present, unnecessary.

The deviation of these latter hectemorial chords might also be investigated, and its maximum determined. The two maxima are not, however, at the same distance from the equator, though not very distant from it. The better method, however, in practice, is to find the value of D for the value of L already determined in the several curves; and then find for this value of D the value of L in the chord. The difference is very nearly the greatest error; which, converted into equinoxial time, gives the error of such a dial reckoned from apparent time. The computations will be given in the subsequent part of the dissertation.

XXXII.

As a proof that DELAMBRE did not even imagine the possibility of finding a co-ordinate equation of the hectemoria, we may quote the last passage which has perhaps been penned—at least the last published—on this subject. It is taken from the "Corrections" to the second volume of his History of Ancient Astronomy. By a system of *experiments* he arrived at some general notion of the form of the curve upon a dial for the latitude of 66 30′; and the advantages of the present system of investigation cannot probably be better shewn than by a comparison with that passage *.

"Depuis l'impression de ce chapitre (that on the Ancient Dials), *pour mieux connaître la figure de ces lignes,* j'ai calculé un cadran pour le cercle polaire; j'en ai déterminé tous les points horaires pour toutes les déclinaisons de degré en degré, et mmeê pour quelques fractions de degré de 23° à 23° 28′; il en est résulté que les lignes horaires pour cette latitude ont à fort peu près la figure du signe d'intégration ∫, c'est-à-dire que dans le voisinage du solstice d'hiver, la ligne a une courbure sensible; que pendant la plus grande partie de l'année, la ligne est sensiblement droite, et qu'elle acquiert de nouveau une corbure, mais moins sensiblement vers le

* Tome ii., at the end of the Table of Contents.

solstice d'été. Au solstice d'hiver la durée du jour est 0; toutes les lignes horaires doivent donc si confondre avec la méridienne. Aux environs de ce solstice les lignes horaires sont si voisines qu'il est presqu' impossible de les distingues, quelque grande que soit l'échelle; en sorte qu'en cette partie le cadrun est aussi inutile que difficile à tracer. Au solstice d'été, au contraire, les lignes sont plus espacées que jamais, parce que le jour est de 24 heures équinoxiales, qui ne font que 12 heures temporaires. La construction est donc très-facile, au lieu que pour l'hiver le meilleur parti est de supprimer la portion de ces lignes que ne peut être d'aucune utilité."

I have thus completed the first part of my plan, that of laying down the mathematical principles of the hectemoria, and investigating those properties of the curves which properly belonged to a solution of the general problem. There yet remains to apply these principles to the construction of the several parts of particular dials, and more especially to examine those remains of antiquity, in which the system of hour-lines either is, or is believed to be, employed. These and some collateral inquiries will form the second part of this dissertation.

On a New Analysis of Solar Light, indicating three Primary Colours, forming Coincident Spectra of equal length. By DAVID BREWSTER, LL.D. F.R.SS. Lond. & Ed.

(*Read* 21*st March* 1831.)

THE decomposition of white light by prismatic refraction, as effected by SIR ISAAC NEWTON, has, for more than a hundred and fifty years, been received as a demonstrated truth by the most distinguished philosophers of all nations. Various attempts indeed have been made, both in his day and in ours, to overturn this beautiful generalization, but they have been made by persons not only ignorant of the subject, but unacquainted with the first principles of physical research.

The analysis of light by the prism is perfect, in so far as it goes, and was demonstrated by NEWTON in the case of spectra produced by the single refracting medium which he employed. It was left, indeed, to his successors to discover the different dispersive powers of bodies, and the irrationality of the coloured spaces, and thus to establish the principles of the Achromatic and the Aplanatic Telescopes. These discoveries presented no points of objection to the views of NEWTON. They were entirely of a supplementary character, and were calculated to establish more firmly his general doctrine respecting the composition of light.

In submitting to the scientific world a new analysis of light, I am fully aware of the difficulties which I have to encounter.

Even in physical science it is an arduous task to unsettle long established and deeply-rooted opinions, and that task becomes Herculean when these opinions are entrenched in national feeling and associated with immortal names. There are cases indeed, where the simple exhibition of new truths is sufficient to dispel errors the most deeply cherished, and the most venerable from their antiquity; but it is otherwise with doctrines which depend on a chain of reasoning where every step in the inductive process is not rigorously demonstrative; and of this we require no other proof than is to be found in the history of Newton's optical discoveries, and particularly in the opposition which they experienced from such distinguished men as Dr Hooke and Mr Huygens.

In the investigations which I am about to explain, the instrument employed is the absorbent action which different bodies exercise on different rays of white light. This principle, which science had hitherto scarcely recognised, was brought into notice by the recent discoveries respecting polarization and double refraction, and was, I believe, first employed as an instrument of analysis, in a paper printed in the Transactions of this Society *. In the experiments there described, I examined Dr Wollaston's spectrum of four colours, viz. *red, green, blue,* and *violet,* by means of a purplish-blue glass. This glass absorbed the *blue* rays, which, when mixed with the *yellow,* made *green,* and the *yellow* rays, which, when mixed with the *red,* made *orange;* and by insulating the *yellow* and *red,* it thus effected a perfect analysis of the *compound green* and the *compound orange.* From this experiment I drew the conclusion *that yellow* light has an in-

* Description of a Monochromatic Lamp for Microscopical purposes, with remarks on the absorption of the prismatic rays by Coloured Media. Read April 15th 1822. Vol. IX. p. 433.

dependent existence *in the spectrum*; and *that the prism* is incapable of *decomposing that part of the spectrum* (of four colours) which *it occupies*. This unequivocal result of a simple experiment at once saps the foundation of the prismatic analysis of light. Sir Isaac Newton, resting on the indications of the prism, concluded that *green* and *orange* were simple colours, and in general, "that to the same degree of refrangibility ever belonged the same colour, and to the same colour ever belonged the same degree of refrangibility;" but it is now obvious that certain *blue* and *yellow* rays, and certain *red* and *yellow* rays, have the very same refrangibility, so that, in the same medium, refrangibility is not a test of colour, nor colour a test of refrangibility.

These views were confirmed by experiments made by Mr Herschel, and printed in the same volume of the Transactions; but, in referring to them five years afterwards, in his Treatise on Light, he regarded them as liable to formidable objections. "This idea," says he (the inability of the prism to analyze light), has been advocated by Dr Brewster, in a paper published in the *Edinburgh Philosophical Transactions*, Vol. XI., and the same conclusion appears to follow from other experiments published in the same volume of that collection. According to this doctrine, the spectrum would consist of at least three distinct spectra of different colours, *red*, *yellow*, and *blue*, overlapping each other, and each having a maximum of intensity at those points where the compound spectrum has the strongest and brightest tint of that colour. It must be confessed, however, that this doctrine is not without its objections; one of the most formidable of which may be drawn from the curious affection of vision occasionally (and not very rarely) met with in certain individuals who distinguish only two colours which, (when carefully questioned and examined, by presenting to them, not the ordinary compound colours of painters, but optical tints of known composition), are generally found to be *yellow* and *blue*."

To these remarks Mr Herschel has added an illustrative figure of the spectrum, in which it is made to consist of *four* colours, *red*, *yellow*, *blue*, and *violet*; the red extending to the middle of the yellow, the yellow beginning at the extremity of the orange and terminating at the indigo, the blue beginning at the middle of the yellow, and terminating at the end of the violet, and the violet beginning at the indigo, and terminating at the extremity of the spectrum.

I have not been able to discover what the general objections are which Mr Herschel refers to in the preceding passage; but the formidable one which he distinctly specifies will be found to have no weight. An obscure physiological fact occurring in one eye out of a million, could not, on any principle, affect the result of a legitimate induction; but even if we invest it with the character of a general fact, it will be found to be a direct argument in support of the very views which it was supposed to contradict.

These views, or the analysis of the spectrum to which they lead, may be expressed in the following propositions:

1. White light consists of *three* simple colours, *red*, *yellow*, and *blue*, by the mixture of which all other colours are formed.

2. The solar spectrum, whether formed by prisms of transparent bodies, or by grooves in metallic and transparent surfaces, consists of *three* spectra of *equal length, beginning and terminating at the same points*, viz. a *red* spectrum, a *yellow* spectrum, and a *blue* spectrum.

3. All the colours in the solar spectrum are *compound* colours, each of them consisting of *red*, *yellow*, and *blue* light in different proportions.

4. A certain quantity of *white light*, incapable of being decomposed by the prism, in consequence of all its component rays having the same refrangibility, exists at every point of the spectrum, and may, at some points, be exhibited in an insulated state.

This remarkable structure of the spectrum will be better understood from Plate II, where Figs. 1, 2, and 3, represent the three separate spectra, which are shewn in their combined state in Fig. 4.

In all these figures the point M corresponds with the *red*, or *least refrangible* extremity of the spectrum, and N with the *violet*, or *most refrangible* extremity; and the ordinates ax, bx, cx, of the different curves MRN, MYN, MBN, represent the intensity of the *red*, *yellow*, and *blue* ray at any point x of the spectrum.

If the distance M x in all these spectra be equal, then, in the combination of them shewn in Fig. 4., the ordinates ax, bx, cx, will indicate the nature and intensity of the colour at any point x of the red spectrum. Thus, let

The ordinate for *red* light		$ax = 30$
	yellow	$bx = 16$
	blue	$cx = 2$
		$a\,x + b\,x + c\,x = 48$ rays,

then the point x will be illuminated with 48 rays of light, viz. 30 of red, 16 of yellow, and 2 of blue light.

Now, as there must be certain quantities of red and yellow light, which will form white, when combined with 2 blue rays, let us assume these, and suppose that white light, whose intensity is 10, will be formed by 3 red, 5 yellow, and 2 blue rays; hence it follows that the point x is illuminated by

Red rays,	-	-	27
Yellow rays,	-	-	11
White light,	-	-	10
			48 rays,

or what is the same thing, the light at x will be *orange*, rendered brighter by a mixture of white light. The *two* blue rays, therefore, which enter into the composition of the light at x, will not communicate any blue tinge to the prevailing colour.

If the point x is taken nearer M, and if, at that point, the *blue* rays are more numerous in proportion to the *yellow* than 2 to 5, that is, if they are as 3 to 5, then there will be 1 blue ray more than what is necessary to make white light with the 2 yellow and the 3 red rays, and this blue ray will give a blue tinge to that part of the spectrum, or will modify the peculiar colour of pure red light. In like manner, the blue extremity of the spectrum may have its peculiar colour modified by an excess of *red* rays so as to convert it into *violet* light. In this manner the tinge of red light at the blue extremity of the spectrum, and of blue light at the red extremity, may be explained, even if the least refrangible branch BM of the blue curve is every where within the least refrangible branch YM of the yellow curve, and the most refrangible branch RN of the red curve every where within the most refrangible branch YN of the yellow curve. On this supposition the excess of blue light over the yellow will begin to modify the red space at that point where the ordinates cx, bx, are in the ratio of 2 to 5; the ratio in which they exist in white light, and the excess of red light over the yellow will begin to modify the blue space at that point where the ordinates of the most refrangible red and most refrangible yellow branch are as 3 to 5, the ratio in which the corresponding rays exist in white light. But it is not improbable that the blue branch BM may actually cross the yellow branch YM at some point m, as shewn in Fig 5; and the red branch RN the yellow branch YN, so that the blue ordinates in the one case, and the red ordinates in the other, will exceed the yellow ordinates at every point beyond the points of intersection m and n. If this should prove to be true, it follows, that, at and beyond n, the red should, as it

were, reappear, and by its predominance convert the extreme *blue* space between *n* and N into *Violet*.

In every part of a spectrum thus composed, there necessarily exist three different colours, which form a compound tint by their union; and as the three differently coloured rays have, at every point, the same refrangibility, it is impossible to separate them, or to analyze the compound tint by prismatic refraction. By transmitting the compound tint, however, through transparent solids or fluids, which absorb one or more of the simple rays, and allowing the rest to pass, we may exhibit one or more of the rays separately, or obtain a residual colour, which indicates the presence of rays whose existence cannot be inferred from the original colour of the compound tint. If, for example, we transmit the compound ray at *x*, Fig. 4, through an absorbing medium, which detains 27 red rays, we shall obtain a transmitted tint with 11 yellow rays and 10 of white light, or a brilliant yellow; and if we again transmit this light through another medium which absorbs 11 yellow rays, we shall have a pure white light, composed of 3 red, 5 yellow, and 2 blue rays. This white light will exhibit the singular property of homogeneous light, namely that of being indecomposable by the prism, and of being pre-eminently adapted for the nicest purposes of vision. The existence of such light has never even been conjectured, and its insulation at any point of the spectrum becomes a proof of the existence, at that point, of red, yellow, and blue rays of equal refrangibility.

Having thus given a general view of the structure which I have found in the spectrum, I shall now proceed to state the experimental evidence from which it has been deduced.

From the simple inspection of the coloured spaces, it is obvious that *red* light exists in the *red, orange*, and *violet* divisions of the spectrum; but, according to Fraunhofer's measurements, these three spaces occupy 190 parts when the whole length of the spectrum is 360; hence red rays are observed in more than one

half of the whole spectrum. If we examine the *blue* and *indigo* spaces through certain yellow fluids, such as *oil of olives*, &c. they acquire a distinct *violet* tint, so that these fluids must have absorbed some rays which had neutralized or masked the red. This violet tint is still more distinct when the spectrum is examined through a pale red glass, which exercises a powerful action upon the rays between *b* and F in Fraunhofer's diagram, combined with a very pale yellow glass. In this case the violet extends to F nearly at the limits of the blue space. The conversion of the whole of the blue and violet space into violet, or what is the same thing, the existence of red light in these spaces, may be exhibited in a very palpable manner by the absorptive action of the yellow juice of the *Coreopsis tinctoria*, when it has been rendered a bright orange red by carbonate of soda, and is used in a state of considerable dilution. Red light therefore exists in the *blue* and *indigo* spaces; and, as I shall afterwards shew that white light, which necessarily includes red, may be insulated both in the green and yellow spaces, it follows that red light exists in all the seven coloured spaces into which the spectrum is divided.

Yellow light is distinctly recognised by the eye in the *orange*, *yellow*, and *green* spaces which occupy 77 parts of the spectrum, whose length is 360. When the spectrum is examined with a deep blue glass, the green light is distinctly seen at F, one of Fraunhofer's lines; and as a green transparent wafer of gelatine produces a whitish band beyond F, and in the blue space, it is clear that a certain portion of yellow light must exist there. We have already seen that the action of oil of olives on the blue and indigo spaces absorbs certain rays and leaves a violet tinge. These rays cannot be red, and they are not blue, because blue taken from blue would not leave violet. They must, therefore, be a small portion of yellow rays, which, forming white with the red, and a portion of the blue, had the effect of diluting the predominant blue light. The existence of both yellow and red rays

in the blue and indigo spaces, may be inferred from another experiment. When we transmit the spectrum through a certain thickness of a blue solution of the ammonio-sulphate of copper, the blue and indigo spaces appear to be much diluted with white light, that is, the blue appears to be mixed with red and yellow. Now, if this apparently diluted blue light is a pure homogeneous blue, containing neither red nor yellow rays, it would suffer no more diminution in passing through an additional thickness of the ammonio-sulphate, than white light would do in passing through the same thickness of pure crystal or pure water, that is, it would suffer no perceptible change. But, in passing through the copper solution, the blue becomes rapidly deeper and less white, which can arise only from its absorbing the red and yellow rays, which cause its apparent whiteness. In order to apprehend the force of this argument, we must consider, that though a dark red or a dark blue fluid appears considerably opaque, as they are in reference to white light, of which the one absorbs all the rays but the red, and the other all but the blue, yet, in reference to red and blue light, which each of them freely transmits, they may be regarded as perfectly transparent. Nothing is more remarkable to those who first make the experiment, than the imperceptible diminution of intensity which a beam of homogeneous red light experiences in passing through a great thickness of a red fluid, particularly when the original red beam is produced by transmission through the same red fluid. It is owing to this cause that the colour of a wine glass, full of port wine, is nearly as deep as that of the wine in the thickest part of a wine decanter.

That *yellow* light exists in every part of the red space, may be proved by numerous experiments. By using a prism of port wine of 90°, or by viewing the spectrum through certain thicknesses of balsam of sulphur, balsam of Peru, pitch, or red mica, *yellow* light can be seen directly at the line marked C of Fraun-

hofer, which is far within the red space; and, by the absorptive action of these four last substances, the whole of the *red* space has a *yellowish* tint, arising from the absorption of *blue* light. The very same effect is produced, but in a more striking manner, by transmitting the light of the red space through certain yellow, orange, and green transparent wafers, all of which absorb some blue light, and leave the whole of the red space of an *orange* tint, that is, containing *yellow* light. In support of the opinion that there are yellow rays in every part of the red space, I may adduce a casual experiment of Sir W. Herschel's, when he had occasion to view the prismatic spectrum reflected from clear turned brass. "The colour of the brass," says he, "makes the *red rays appear like orange*, and the orange colour is likewise different from what it ought to be *." From these observations it follows, that yellow light may be traced through all the coloured spaces except the violet, where I have not yet been able to find it; but this is not surprising when we consider the great faintness of the violet rays, and the facility with which they are absorbed by media of almost all colours. Even the deep blue ammonio-sulphate absorbs almost the whole of the violet space, and the smalt blue glass nearly one half of it, so that it is extremely difficult to subject it to the partial action of absorbent media.

It is obvious, even to the eye, that *blue* light exists in the violet, indigo, blue, and green spaces, which occupy 247 parts out of 360, or more than two-thirds of the whole spectrum. When the most refrangible rays are absorbed by certain thicknesses of balsam of sulphur, balsam of Peru, pitch, or red mica, the blue mixed with yellow, and forming green, may be traced very near the line C of Fraunhofer, which is considerably within

* Philosophical Transactions, 1800, vol. xc. p. 255.

the red space. That the blue extends over the whole red space is proved by the same arguments which we used for yellow light; for when the red space is made of an orange tinge by the absorptive action of certain yellow, orange, and green media, this change can be effected only by the absorption of blue light.

Having thus proved that *red*, *yellow*, and *blue* light exist in almost every part of the spectrum, I shall proceed to corroborate these views by shewing that *white* light may be actually insulated in different parts of it.

When we look at the spectrum through a particular blue glass of a certain thickness, we insulate the yellow space, the colour of which is a rich gamboge-yellow. By increasing the thickness of the glass, this compound yellow acquires the pale straw-yellow colour of the yellow monochromatic flame produced by the combustion of alcohol and water, or of an alcoholic solution of salt. At a still greater thickness of the glass we produce a *greenish-white* band, which, by changing the glass for a different blue, becomes a *reddish-white* band. If we now mix a solution of sulphate of copper, which acts upon the rays on the red side of the yellow space, with diluted *red* ink, which acts on the rays on the *blue* side of the same space, we shall reduce the rays in the yellow space to nearly *white light*, with a slight tinge of *green*, when there is too much *sulphate of copper*, and a slight tinge of *red* when there is too much *red* ink. This insulation of white light may be pretty well effected by some of the smalt blue glasses acting alone; and in some cases the purity of the light may be increased by a solution of *sulphate of copper and iron*, or even by a *green glass*. The yellowish-green juices obtained from the green leaves of plants, and likewise some of the *lilac* juices, such as that of the black currant, will be found useful in rendering whiter this luminous band when produced by blue glass. The white light thus exhibited may be rendered

yellow by means of a *yellow* transparent wafer, which absorbs some of its *blue* rays, and *green* by a *green* transparent wafer, which absorbs some of its *red* rays.

From these experiments it follows, that *white light*, composed of *red*, *yellow*, and *blue* rays, exists in the most luminous part of the spectrum, on the most refrangible side of the line D, and may be insulated by absorbing the excess of yellow light, and of any of the other colours above what is necessary to compose white light. In applying a highly dispersing prism, it was a singular and peculiarly interesting sight to witness, for the first time, a beam of white light, consisting of *red*, *yellow*, and *blue* rays of equal refrangibility, and incapable of being analyzed by prismatic refraction.

The preceding observations contain only a few out of a great number of experiments which I have made on the absorptive action of natural and artificial crystals, and of various fluids and uncrystallized solids which possess either a natural or an artificial colour. I made a few experiments with the coloured juices of several hot-house plants, which Mr Forbes was so good as to prepare for me; and I expected, in the course of the summer, to obtain by this means, a more striking insulation of some of the simple colours, than I had effected by the substances within my reach. Impatient, however, of so long a delay, I thought of supplying the place of these absorbing fluids by forming the spectrum out of common or polarized light, which had undergone the periodical action of thin plates in crystallized laminæ, or, what is the same thing, by examining the spectrum through such films and laminæ which have the property of transferring to the reflected pencil certain alternate portions of the spectrum, while all the intermediate portions appear in the transmitted rays. By this means I was enabled to divide the spectrum into any number of stripes, alternately obscure and coloured, so that each of

the coloured portions could be separately submitted to the action of absorbent media.

In its practical applications, this new principle exceeded my most sanguine expectations, and has furnished me with the means, not only of analyzing the colours of natural bodies, but of determining the causes from which these colours originate. An account of these, and of other applications of it, will form the subject of separate communications, and I shall confine myself at present to the single observation, that, in applying this method of absorption to the decomposition of the solar rays, I have been able to insulate white light, both in the *orange* and in the *green* space, and thus obtain the most ample proof of the peculiar analysis of white light which it has been the object of this paper to establish.

By means of this analysis we are now able to explain the phenomena observed by those who are insensible to particular colours. The eyes of such persons are blind to *red* light; and when we abstract all the *red* rays from a spectrum constituted as above described, there will be left two colours, *blue* and *yellow*, the only colours which are recognised by those who have this defect of vision. To such eyes light is always seen in the red space, but this arises from the eye being sensible to the *yellow* and *blue* rays which are mixed with the red light.

Hence blue light will be seen in the place of the *violet*, and a greenish-*yellow* will appear in the *orange* and *red* spaces, or what is the same thing, the spectrum will consist only of the *yellow* and the *blue* spectra shewn in Figs. 2 and 3. The physiological fact, and the optical principle, are therefore in perfect accordance; and while the latter gives a precise explanation of the former, the former yields to the latter a new and an unexpected support.

EXPLANATION OF PLATE II.

Fig. 1. Represents the ***Red*** spectrum, which enters into the composition of the solar spectrum, and extends along the whole of it. The maximum intensity is opposite R.

Fig. 2. Represents the *Yellow* spectrum of the same length. Its maximum intensity is opposite Y.

Fig. 3. Represents the ***Blue*** spectrum of the same length. Its maximum intensity is opposite B.

Fig. 4. Represents the ***Solar spectrum,*** formed by the union, or superposition, of the three preceding figures.

This figure is intended merely for illustration, and not as an accurate picture of the solar spectrum. The proportion of the coloured spaces is, however, very nearly that which Fraunhofer determined by examination through a telescope. The fixed lines discovered by this eminent philosopher are omitted.

Fig. 5. Differs from Fig. 4 only in the relative forms of the three curves R, Y, and B, whose ordinates are supposed to represent the intensity of light at different points of each of the three simple spectra. Their form is entirely empirical, though it cannot be very different from the truth. Fraunhofer has determined from observation the general curve of illumination for the whole solar spectrum; and though there is no method of determining the ratio of the three ordinates, ax, bx, cx, Fig. 4. at any point of the spectrum, yet the sum of these ordinates, $ax + bx + cx$, must always be equal to the ordinate of the general curve at the same point x, as determined by Fraunhofer.

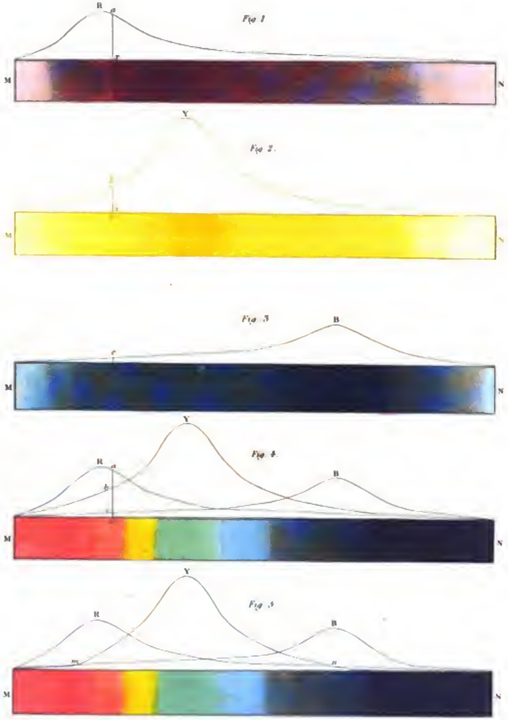


Fig 1
R
M
N
Y
Fig 2.
M
N
Fig 3
B
M
N
Y
Fig 4.
R
B
M
N
Y
Fig 5
R
B
M
N

Notice regarding some Experiments on the Vibration of Heated Metals. By Arthur Trevelyan.

(Read 17th January and 21st March 1831.*)*

In the month of February 1829, I discovered accidentally that a bar of iron, when heated and placed with one end on a solid block of lead, in cooling vibrated considerably, and produced sounds similar to those of an Æolian harp.

1. I have lately made further experiments with bars of copper, zinc, brass, and bell-metal, which, when heated and placed on blocks of lead, tin, or pewter, produce the same effect as the iron.

2. The bars I generally used in making the experiments, were 4 inches long, 1½ inch wide, and 1½ inch thick, with a rod 7 inches long attached to one end, to serve as a handle. A ridge is formed along the centre of one side of the broad part, by its being bevelled off towards each edge; the other side is flat. The longitudinal ridge is the part that rests on the block.

3. A notch in the lead block, or a groove made along the ridge which runs up the centre of the bar on the part where it rests on the block, increases the sounds.

4. The notes on two bars that I have tried, of different sizes, at different temperatures, are in the key of E major, with four sharps, commencing frequently with B natural in the bass.

5. If the vibrating bar be placed on a piano-forte, and certain notes be struck on the instrument, the vibration of the bar, and

consequently the tone produced by it, is altered, and sometimes ceases altogether.

6. A common poker, heated and placed on a lead block, vibrates, producing deep tones also in the key of E major.

7. By balancing the bar in a horizontal position on a narrow lead block, rounded on the part upon which the bar rests, the vibration is displayed much more distinctly; the handle of the bar moves vertically, and the part resting on the block laterally also.

8. A rod 10 or more inches long, flattened in the centre to prevent it slipping, with a ball at each end, when placed across a heated vibrating bar, and increasing the motion, renders, by the length of its arms, the lateral movements much more conspicuous.

9. The longer the rod is, the slower are the movements of the heated bar, but its oscillations are so much the greater.

10. A thick ring of copper, 5 inches in diameter, when heated and hung on a lead bar, vibrates backwards and forwards; and when laid on a narrow block of lead, upwards and downwards.

11. The heated bars vibrated on a piece of thin sheet-lead, either placed loosely or soldered on a block of hard metal, and on a lead block burnished with gold-leaf.

12. The vibration continues in the exhausted receiver of an air-pump.

13. The brass bar, after being heated in boiling water vibrated when placed on the lead.

14. A copper bar vibrated when heated and placed on cylinders of brass, or of an alloy of copper with an edge $\frac{1}{16}$th of an inch in thickness; also on the bottom of a copper box about $1\frac{1}{2}$ inch in diameter, and slightly cupped out, leaving a ridge all round, on which the bar rested.

15. The same effect was produced on the bottom of a glass tumbler, but these effects are frequently difficult to be obtained.

16. The bars vibrate best when placed on blocks of lead with the surfaces somewhat rough. Both metals should be also kept clean, and free from oxidation, which impairs the vibration.

17. A short bar of heated copper was placed on an iron block rounded on the surface, and being nicely balanced on the centre of the rounded part, showed the vertical motion.

18. When the blocks have acquired nearly the same temperature as the bars, the sound and motion cease.

19. I could not perceive the slightest vibration when the heated bar was placed on a small block of platina.

20. Vibration is prevented by rubbing the surface of the lead block with mercury, oil, plaster of Paris, or by oil-gilding; and also if a piece of thin writing paper be interposed between the bar and the block.

21. Air blown with a pair of bellows on the heated bar when vibrating, does not affect the tremors.

22. By means of a frigorific mixture, I cooled the lead block to 8° F., and placed on it a brass bar at the temperature of the air in the room, but no vibration took place.

23. I have not observed any vibration of the heated bars when laid on solid blocks, either rough or smooth, of copper, zinc, antimony, brass, marble, plaster of Paris, or clay, or on lead with a piece of sheet-brass soldered on it, although I have tried bars and blocks of different sizes and shapes.

24. Bars made of pewter, tin, or lead, heated and placed on the cool blocks of either the soft or hard metals above named, do not vibrate.

45. A cool bar of lead, placed on a heated block of brass or copper, vibrates.

26. A block of lead, laid on the flat upper bar of a heated grate, shows the vibrations, accompanied with sound.

27. The heated bar was placed upon a block of copper, in which a number of holes, $\frac{1}{8}$th of an inch in diameter, and $\frac{1}{4}$th of an inch deep, were drilled, but no vibration took place.

28. If the heated bar be ground smooth on the resting part, and the block of lead be also very smooth, no vibration takes place.

29. If a hard-metal bar be placed on a thick lead cylinder, or on a lead block, with the end overhanging, and a spirit-lamp under it, it soon becomes sufficiently heated to produce both vibration and sound, which I have kept up for an hour and a half.

30. The shape of the bars and blocks is of little consequence, as the hard metal of almost any form vibrates when heated and placed on a lead block.

31. If thin bars of metal be employed, sound is often produced though the tremors are not visible.

From the above related experiments, it appears,—

1st, That, in order to produce the vibrations, two different metals must be employed, the one soft and possessed of moderate conducting power, viz. lead or tin, the other hard; and they take place whether soft metal be employed for the bar or block, provided the soft metal be cold, and the hard metal heated.

2d, That the difference of temperature between the bar and the block must be considerable, though the exact amount has not been rigidly determined; it has succeeded with a difference of 170°.

3d, That the surface of the block shall have a certain degree of unevenness, for when rendered quite smooth, the vibration does not take place; but the bar cannot be too smooth.

4th, That the interposition of any matter prevents vibration, with the exception of a burnish of gold-leaf, the thickness of which cannot amount to the two hundred thousandth part of an inch.

5th, That the air has no share in the production of the vibratory movements, however much its presence is essential to the production of sound.

The cause of the vibrations is by no means obvious. I shall take the liberty of mentioning those speculations or hypothetical

views which have presented themselves, though with diffidence and brevity.

1*st*, As the atmospherical air is thrown into currents when unequally warmed, it may be supposed that the hot bar induces such statical movements, as prove sufficient to occasion its vibrations.

I need not attempt, however, to explain in what manner the currents of air could produce the tremors, seeing that, by Experiment 12, &c. it is proved that the air has no concern in the vibratory operation.

2*d*, As the thermo-electric experiments and discoveries of late years have shown, that when different metals in contact with each other, differ considerably in their temperature, they acquire and exhibit opposite states of electrization, it may be supposed that as soon as the heated bar is laid upon the cold block, the electric equilibrium is disturbed, and that the metals assume the positive and negative conditions. A series and succession of electric attractions and repulsions may be conceived then to commence, which by frequent and rapid repetition cause the commencement and continuance of the tremors.

3*d*, A third hypothesis, and which appears to me the most probable, ascribes the vibratory movements to the usual mechanical changes which caloric occasions in passing from one substance into another: I mean the expansion and contraction which accompany alternations of temperature. It is shown by several of the experiments above recorded, that one of the two metals must be a soft one, as lead or tin. Both of them possess in a smaller degree the power of conducting caloric than the harder or more elastic metals which were used for the bars.

It also appeared, that some degree of roughness of surface of the lead block is essential to the success of the operation. This slight asperity arises of course from numberless points or ridges projecting from the solid mass of metal.

When the heated bar is laid upon the leaden block, the caloric passes into these prominences; and as their power of conduction is not great, it does not rapidly diffuse itself through the rest of the mass; of course, they instantly expand and elongate, and by that sudden elongation they give an impulse to the incumbent bar. Soon, however, the caloric moves into the adjoining mass and the prominences contract, and at the same time come into a state ready to admit a renewed accession of caloric from the bar; they receive that caloric; again expand, and give a second impulse to the bar. This goes on incessantly; and though the first impulse be infinitely small, and altogether inadequate to produce any sensible movement of the bar, yet, by incessant repetition, an accumulation of effect takes place, and the movements gradually reach a magnitude sufficient to become easily discernible.

As soon as the bar and block arrive within a certain limit of difference of temperature, the impulses become feebler and feebler; and at length the bar comes to rest.

It has been mentioned, that the smoother the bar is, so much greater is the effect. I conceive that this smoothness operates by increasing the celerity with which the surface possessed of it communicates the caloric to the projecting points of the block, and thereby the elongation which gives the impulse to the bar is increased both in quickness and extent.

It is obvious, that had the bar any considerable degree of asperity, the points of contact between the two metals would be fewer, and the passage of the caloric between them more tardy.

When the surface of the block is highly polished, the experiment does not succeed; no tremors occur. This result proceeds probably from the circumstance, that the caloric enters into every part of the surface of the block equally, and is more quickly diffused through the mass, and hence there are none of

those partial and sudden expansions which give the tremor-causing impulses.

When the bar lies with a flat surface upon the block, the movement of course takes place in a vertical position only; but when the bevelled face of the bar is presented to the block, and the bar rests on the longitudinal ridge, then, along with the vertical, the transverse vibration also occurs, and communicates the rocking movement.

This rocking may be induced by two causes; either by some slight inequality in the weight of the portions of the bar on the two sides of the ridge, or some difference in the condition of the surface of that part of the block which the ridge of the bar touches.

1*st*, If the first mentioned inequality exist, as soon as the bar receives an upward heave, the greater weight of the one side will cause it to incline to that side, and as soon as that heave ceases, and the contraction succeeds, the bar approaches its original position, but will not remain in it, for the inclination given to the preponderating side will, on its return, of course, cause the bar to incline to the opposite side.

The impulses which the bar receives in this position from the renewed expansion and elongation, will not only renew the upward heave, but also give it a shove to the preponderating side, and thus increase the lateral movement, which, like the vertical, though altogether insensible at first, by incessant frequent repetition accumulates and increases till the rocking becomes conspicuous, and is rendered much more so by the transverse rod.

2*d*, If there exist any difference in point of asperity, in the condition of the surface of that part of the block upon which the ridge of the bar rests, it must necessarily follow, that the impulse given to the bar on that side which is the most rough, will be greater than on the other, and consequently the upward

heave will be so modified as to create an inclination to one side. The bar thus thrown off its balance to the right on ascending, will incline as far to the left on descending, and there receiving the expansive impulse, it will be driven back, and thus the principle of rocking will be created.

With regard to the sound, which constitutes the second portion of the remarkable phenomenon, the following remarks have presented themselves. It seems indispensable to its production that there be an uneven surface, along which a current of air may pass between the bar and block: this roughness may exist either in the block or in the ridge of the bar.

The manner in which the sonorous undulations of the air, which, reaching the internal ear, cause the sensation of sound, may be occasioned, is not very manifest.

By attending to the usual modes in which acoustic aërial pulses are produced, we may perhaps be able to assign the one which proves the source of the sound in our experiment.

The first mode is, when an elastic substance, thrown into vibration by an impulse given to it, communicates the tremor to the ambient air, as when a bell, or a simple musical instrument, for instance the stoccado, is struck.

The second is, when the sonorous tremor is acquired by the air passing through one or more apertures varying in size, and with varying degrees of force and velocity. I consider the animal voice of this description; for though some physiologists maintain that the voice is produced in the third mode, immediately to be mentioned, I am disposed to think, that the sound, and all its modulations, depend upon the form and condition of the aperture of the glottis, and of the passage through which the expired air passes. For though the numerous cartilages, and more especially the arytenoid, may be considered as vibratory bo-

dies, yet the very soft mucous texture which covers them appears to be intended for the very purpose of preventing or extinguishing any tremor which might arise. Could we suppose a flute, or any wind-instrument, constructed of inelastic earthenware, we should have a perfect source of that aërial vibration to which I am now alluding, and such I conceive the organs of the animal voice to be.

Thirdly, When air, anyhow thrown into tremors, impinges upon an elastic solid, and throws it into vibration, the tremors of the solid body are in their turn communicated to the air in contact, and the sonorous pulses may be considered as a complex modification, resulting from the intermixture or semi-combination of the two sets of tremors.

The acoustic tremors of ordinary wind-instruments are of this description. They are finely illustrated by the wild Æolian tremulous notes emitted by the cylindrical glass-tube in which a very slender jet of hydrogen gas is made to burn; also by the very loud notes issuing from the gas-lamp furnace now employed in chemical experiments, when a chimney eight or ten inches high is placed upon them.

These three are the more common methods of exciting sound-causing pulses; but, in our experiment, the source of the sonorous undulations of the air appears to be different. In it we have a solid thrown into vibration by the peculiar mode of the passage of caloric from one mass of metal into another, and necessarily occasioning tremors in the contiguous air; and we have, at the same time, the air in undulatory movement, induced by flowing in a current through the channels afforded by the roughness of the block or bar; but to produce a soniferous undulation, neither of these singly is sufficient. It is the combination which causes the result.

This appears to me to be demonstrated by the facts, 1*st*,

That the tremulous metal causes no sound when the two surfaces are smoothly polished, as there is then no opening for the stream of air; and, *2dly*, that the current of air occasions no sound when there is no vibration of the solid, seeing no sound is heard when a heated bar of lead rests upon a block of the same metal, and when no vibration is visible.

I shall not lengthen this paper by attempting to explain the disposition of the apparatus to yield notes of particular intonation.

A Description of a Fossil Tree discovered in the Quarry of Craigleith, near Edinburgh, in the month of November 1830; *with a Short Account of a Fragment and Branch found in* 1831. By HENRY WITHAM, Esq., F.R.S. Ed., F.G.S., &c. &c. &c.

(*Read 21st February* 1831.)

IN the month of November 1830, a small portion of a magnificent fossil stem was discovered in the quarry of Craigleith, in this vicinity, and more and more of it was brought into view as the working of the quarry proceeded.

In December following, I communicated to the Natural History Society of Northumberland a description of that part of the petrifaction which had then been uncovered. This description will appear in their Transactions.

At that period a considerable part of the stem had been brought into view, and since that time the remaining portion, and the roots, have been exposed.

From the interest which this discovery has excited, and the numerous visitors which it has attracted, I hope a record of it will not be unacceptable to the Royal Society of Edinburgh, in the near neighbourhood of which this object lay imbedded, and the parts of which, after careful disinterment, are now open to the inspection of the curious, one half having been deposited in the Museum of the College, and the other in the Royal Botanic Garden.

The accompanying Plates are intended to afford a more precise idea of the position, form, and internal structure of this fine petrifaction than could be conveyed by description. Plate III, from the pencil of Dr GREVILLE, represents a portion of the fossil in its natural position. Plate IV. represents the internal structure. Plate V. is a sketch of the trunk, including its supposed continuation. These two plates are from drawings by Mr MACGILLIVRAY. Plate VI, also by Dr GREVILLE, is a view of that part of Craigleith Quarry in which the fossil tree was found.

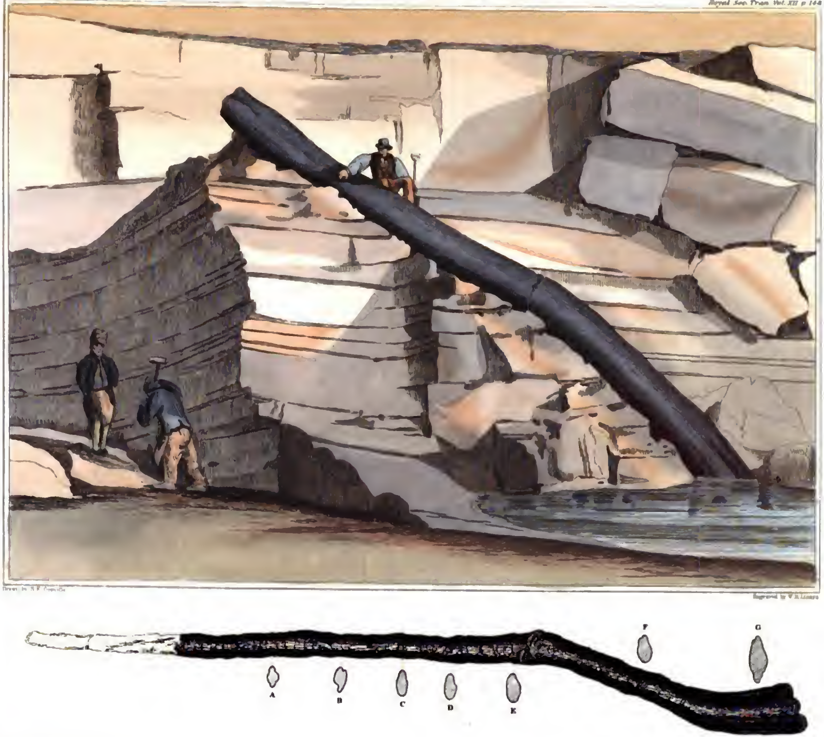
Craigleith quarry, in geological position, is situated in the mountain-limestone group, and lies considerably below the great coal basins of the Lothians. Its elevation above the medium level of the sea, by barometrical measurement, is 75 feet. In the part of the quarry in which the fossil is situate, the strata incline to the NN. E. one foot in 4½.

This part of the quarry, from some unknown cause, assumes a trough-like shape, the one side dipping at an angle of 20° to the south, and the other side at an angle of 20° to the north; at the bottom of this basin lay the roots of this splendid fossil. The general direction of the tree is 20° W. by N., and the dips are as follows: A. B, Plate V, at an angle of 20°, B. C. 44° 5′, C. D. E. 39° 35′, F. 28°, G. 28°. The length of the stem, from the top to the root, was 47 feet. It presents the appearance of a large branchless trunk, in some parts greatly flattened, so as to form an elliptical section. The diameters are nearly as follows, and will best shew the places and proportions of such flattenings. See Plate III.

	Feet.	Inches.		Feet.	Inches.	
A.	1	7	long diameter, and	1	4	short diameter.
B.	1	10	do.	1	3	do.
C. D. E.	2	7	do.	1	0	do.
F.	2	8	do.	1	5	do.
G.	5	0	do.	2	0	do.

PLATE III. FOSSIL TREE AT CRAIGLEITH QUARRY.

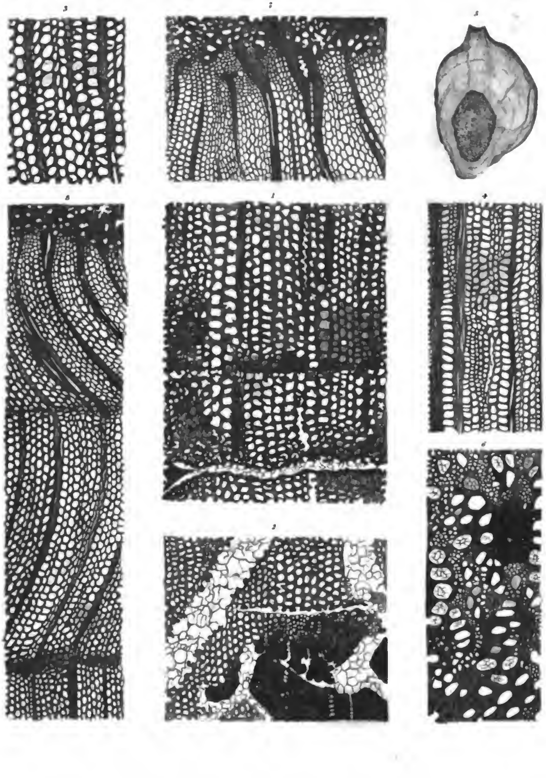
Royal Soc. Tran. Vol. XIII p. 148



Drawn by S. F. Greville

Engraved by W. H. Lizars

PLATE IV.



MAGNIFIED SECTIONS OF FRAGMENT OF BRANCH OF FOSSIL TREE.
Discovered in Craigleith Quarry in 1831.

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A	Dip	30°	Diameter	5 ft by 3 ft	
B		40° 4		2 0 by 1 8	
C		30° 35°		3 0 by 1. 8	
D		30° 35°		3 7 by 2	
E		30° 35		3 7 by 2	
F		20° .		2 10 by 1 3	
G		20° .		1 7 by 1. 4	

H Supposed prolongation of the top of the stem 12 feet

I. Upper part of the tree first discovered, showing a cavity the seat of a branch

Length from A to G 47 feet

FOSSIL TREE IN CRAIGLEITH QUARRY.

Discovered Nov[r] 1830

Mac Gillivray

Engraved by W.H. Lizars

PLATE VI.

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On the edges of A was not more than 7 inches. From the diameter of the highest part mentioned, it appears quite evident that many feet of the top part of the stem must have been taken away unobserved, ere it attracted the notice of the public, leaving fair ground to conclude, that, when it waved in the winds which whistled through its spreading branches, for aught we can say, a million years ago, its magnificent trunk stood tall and stately full 60 feet in height. Judging from the unworked rock near where the stem lies, the superincumbent mass must have been above 100 feet. The stem tapers gradually, and is marked at irregular intervals with a kind of transverse rugæ, or irregular prominences. The bark has been converted into coal, and presents indistinct longitudinal markings, with very small transverse ridges. At some of the prominences, the rugæ are contorted like the coming off of branches of various trees. That the forms of fossil trees are frequently much altered prior to their consolidation, or during the process of petrifaction, there can be no doubt. The shapes themselves, and the circumstances in which they are discovered, often sufficiently bear evidence to the fact, that pressure and other agents have been at work. But recourse has often been had to this mode of explanation in cases where it could not apply, and where one much more easy was at hand. It is by no means uncommon to find amongst recent trees, forms similar to those of fossil plants, which have been ascribed to pressure and other external causes. Every one who has had the opportunity, and has availed himself of the occasion, must have observed this. The flattening, therefore, of this fossil tree, is only similar to what exists in living nature, although I am unwilling to believe that to be the case in this splendid instance of early vegetation. The usual way of accounting for such flattenings is by pressure, although in the present case, where the tree is not parallel to the strata, it is rather difficult to suppose its form to be owing to that cause. The pressure

by loose sand, or by sand mixed with water, would act all round the stem, and so would not flatten it; but if we suppose that the tree in its recent state was carried along by a torrent of water and sand, and left sticking as the latter consolidated, it would afterwards begin to decay, when the hardened strata would necessarily press down upon it, and so produce the flattening; and those parts of the stem which decomposed rapidly, would naturally shew the effect of pressure most; and such may have been the case with the fossil stem under consideration.

In the great coal-field of the north, fossil plants are generally found in a horizontal position, or parallel to the strata, in the greatest possible confusion, much broken, much compressed, and with their parts far separated. Indeed, the confusion is the most serious difficulty the observer has to contend with. It is, however, by no means easy to trace the operation of a current of water that has swept off weaker vegetables, and deposited them where we now find them so beautifully preserved.

Notwithstanding this, there are to be found in considerable abundance, in various positions, large and strong trunks of plants which appear to remain in their natural positions, and which have been able to withstand the force of such torrents, if it can be proved that any such existed. These vertical plants are generally found to be Sigillariæ. The Stigmariæ of Brongniart (the Lepidodendra of Sternberg), and the Equisitaceæ, on the contrary, do not appear to have been sufficiently strong to have resisted such revolutionary influence. Several scientific gentlemen having stated as their opinion that this fossil is a Lycopodium, I may here mention the reasons why I have come to a different conclusion. 1*st*, In this plant there are no appearances of insertions of leaves on any part of it, or any markings similar to the scales of Palms, or Ferns, or the imbricated leaves of the Lycopodia. Judging alone from external appearances, the probability is, that it is the stem of a dicotyledonous or gymnospermous

phanerogamic plant. *2dly,* Having examined with care the internal structure of this fossil tree under the microscope, agreeably to the rules laid down in my Observations upon Fossil Vegetation, I find it cannot belong to the former of these classes It has, however, most decided medullary rays, and a woody texture, with some appearance of concentric circles, and must therefore belong to the order Coniferæ. (See Plate IV. Figs. 1 and 2.) It cannot be a Cycas or a Zamia, and a Lycopodium it cannot possibly be, these plants being vascular cryptogamic, composed of cellular tissue withscular fibres, destitute of medullary rays, concentric rings, or woody texture, and generally dichotomous. Great numbers of these gymnospermous phanerogamic plants have lately been discovered in the shales of the mountain-limestone groups, much broken, and lying in a state of great confusion. This gigantic stem so far exceeds in size the generality of those found in similar situations, that although possessing, in common with the rest, the same general character, yet I fear, with our present limited knowledge in this hitherto neglected field of botany, it would be dangerous, or rather impossible, to name the genus. I may here mention, that the concentric rings so very apparent in plants belonging to the oolitic series, are not in general so conspicuous in those found in the mountain-limestone series and coal-fields; probably, amongst other causes, owing to the abundance of calcareous matter, and the peculiarity of crystallization; yet, from the examination of the few slices of this tree already cut, I feel persuaded that they will be found in many parts of its magnificent trunk.

Since commencing this short account, a fragment of a third fossil stem, with a branch, has been discovered on the south side of the same quarry, between where the fossil discovered in the year 1826, and that found in 1830, lay. This fragment and branch were imbedded in one of those indurated parts of the rock called by

the workmen *Kettle bottoms*. The rock being so hard, it became necessary to blast it, which probably detached the branch from the fragment of the stem. The branch has been sliced, and shews the concentric rings in a perfection almost approaching to the fossils belonging to the oolitic series. (See Plate IV. Figs. 3, 4, 5, 6, 7, and 8.) The pith is large. This is the most powerful link which has been found to unite the chain of evidence, which, in every other respect, is so strong in favour of these plants belonging to the phanerogamic class. Two vast Coniferæ and a fragment found within 400 yards of each other, afford strong reason to think, that, in a square mile of the same deposite, many of these ancient relics of early creation will be brought to light, and leave room to believe that these phanerogamic plants are as abundant in these deposites as in those higher up in the series*.

* In August 1831, another portion of a fossil has been discovered within a few yards of the fragment of the third fossil stem. An horizontal slice of this, 4 inches long and as many broad, shews the concentric zones of the wood in a very marked manner; and a longitudinal section of the same exhibits distinctly the circular elevations, equal to about half the breadth of the elongated cellules. Here, then, the very remarkable organization of the walls of the woody tissue of recent Coniferæ are exhibited more satisfactorily than in any of those plants hitherto discovered in or below the coalfield proper.

On the Horary Oscillations of the Barometer near Edinburgh, deduced from 4410 *Observations; with an Inquiry into the Law of Geographical Distribution of the Phenomenon.* By JAMES D. FORBES, Esq. F.R.S.E.

(Read 4th April 1831.*)*

1. THE science of Meteorology must be ranked, at the present moment, among the most rising branches of natural knowledge. The transition from the hasty generalization which always marks the embryo state of science, to the application of sober inductive analysis, is one so important, and so truly interesting, as to repay amply the philosophical abstinence which it imposes. No more important lesson, indeed, can be learned, than from the very examples of crude speculation, which, for centuries, the progress of this subject has afforded among the multitudes whose scientific acquirements are limited to the art of consulting a weather-glass, or registering a thermometer, little imagining that the very science they affect to cultivate, ranks among its phenomena the interwoven effects of remote and recondite causes,—a science which, to use the words of Mr HERSCHEL, is "one of the most complicated and difficult, but, at the same time, interesting subjects of physical research: one, however, which has of late begun to be studied with a diligence which promises the speedy disclosure of relations and laws, of which, at present, we can form but a very imperfect notion."

2. One of the most important features of recent improvement in the mode of studying the group of facts which meteorology

presents to us, is the analytical method of discussing observations, extracting, as it were, from the data which nature presents to us, laws which shall best represent them, instead of framing clumsy hypotheses, to which it became a secondary object, to apply the facts. No better example of the method can be given, than Humboldt's masterly essay on Isothermal Lines; and the same distinguished traveller was the first who threw into anything like lucid order, the class of phenomena which it is the object of this paper to discuss,—the Horary Oscillations of the Barometer.

3. These atmospheric tides were first discovered in a decisive manner at Surinam, in 1722, by a Dutch philosopher, whose name has not descended to us; and the history of their subsequent development is too well known to require even an abstract in this place. It has been reserved for the nineteenth century to ascertain almost every thing connected with them besides their bare existence, and even that was not established in the temperate zone previous to the observations of Ramond, little more than twenty years since. But it was reserved for Humboldt, after much practical labour in the first years of the century, to draw up the admirable analysis of the subject which appeared in the third volume of the "Relation Historique," published in 1825. From that time a new impulse was given to the labours of observers; and during the last five years many important registers have thrown light upon the modification which the barometric oscillation undergoes in different latitudes, and at various heights above the sea. The subject had always appeared to me one of singular interest, and accidental circumstances first induced me to prosecute it experimentally, during some months' stay at Rome in the spring of 1827. The period was very short, but as I frequently made from twelve to fifteen observations in a day, I was enabled, very satisfactorily, to trace the variation from hour to hour, and to ascertain the periods of maximum and minimum, though too brief to determine the amount accurately, which came

out, in all probability considerably too small *. On my return to Scotland in autumn of the same year, I resolved to institute immediately a series of observations in Latitude 56°, a more northerly station than any at which observations have, as far as I know, yet been made for this purpose, excepting those of Captain Sir Edward Parry. These I have pursued almost without intermission, up to the close of 1830; and when it is recollected how rare observations, sufficiently long continued, have been in the higher latitudes, and especially how trivial have been the donations of Great Britain to this branch of meteorology, I hope that, with all the defects inseparable from the efforts of a solitary observer, the results of this inquiry, placed in the form of Tables at the end of this paper, and the deductions from which I am now about to give in words, may not be unacceptable to the Society.

4. These observations, amounting in all to 4410 in number, comprised between the years 1827 and 1830, were made at Colinton House, four miles south-west of Edinburgh, in Latitude 55° 55′ 20″ N., and Longitude 12^m $57^s.5$ west of Greenwich, deduced trigonometrically from the Calton Hill Observatory, and at 410.5 feet above the mean level of the sea, determined by accurate levelling. Four months observations, however, at the commencement of 1828, were made in Edinburgh, but reduced to the same level of 410.5 feet, in order to prevent their affecting the mean absolute height. Five observations were made daily of the barometer and attached thermometer, from 8 to 8½ A. M., at 10 A. M., about 4 P. M., and at 8 and 10 P. M.; in order to detect the morning and evening maximum, and afternoon minimum. The same instrument has not been used throughout these observations. For the greater part of the last two years, however, I have used a barometer in which I put great confidence, the mercury having been boiled in the tube by my-

* A full account of these observations was published in the *Edinburgh Journal of Science* for January and April 1828.

self with every precaution, and with every part of which I am thoroughly acquainted. It is of the mountain construction, has an adjustable level, and attached thermometer, and requires no correction for capillarity. Though the other barometers were not so unexceptionable, they were both furnished with attached thermometers, and one was on the mountain construction; indeed the nature of the deductions which I have to make being entirely confined to *differences*, without regard to absolute height, the nicety of the instrument is of little importance, considering the great number of observations required to be combined, in order to obtain any trust-worthy result. Any constant cause, rendering the height of the mercurial column at one hour of the day different from that at another, independent of the atmospheric tide, must be sedulously guarded against; but there is nothing worth mentioning which can produce this effect, except change of temperature. The attached thermometer must therefore be considered a *sine qua non*. The corrections on this account are applied to the monthly means; and the table I have employed is that of Professor Schumacher: it may safely be affirmed, that, with rare exceptions, the tables commonly employed in this country are more or less erroneous, and some very strikingly so. The whole of the observations are thus reduced to 32° Fahr. I may also add, what is by no means unimportant, that with hardly any exceptions (amounting to not above a few dozens in all), the whole of these 4410 observations were made personally by myself.

5. In the numerical reduction of the observations, I experienced a very harassing difficulty. The omissions at particular hours, which could not fail to occur occasionally, in the attempt of an individual to register the barometer five times a-day for so long a period, where they happened during extreme states of pressure, would, it is easy to see, materially affect the monthly means; and although the reduction was continued in the hope that the combination of a sufficient number of months would destroy the irregularity, it became obvious, that any attempt to

draw more delicate conclusions, such as the influence of the seasons, would be vain; and, finally, I was led to distrust the accuracy of even the general result. Under this impression, I resolved to repeat the whole reductions upon a plan which I had previously employed with success, and which has fully answered my expectations. As the great object was to avoid the inequality of absolute height, which might vary indefinitely on different days, and preserve only the differences corresponding to different hours, I selected one time of day at which the barometer was most regularly observed, and, under the columns corresponding to the other hours, inserted the differences as they were + or — of the barometer and attached thermometer, from the heights at the standard hour inserted in the first column, and the means for each month of the principal column and the subsidiary ones being taken, the corrections (total or partial respectively) for temperature were applied, from which mean results a new set of absolute heights might be deduced. By far the greater deviations from the mean, on the account above alluded to, were thus avoided, and though particular omissions no doubt affected the partial means, the errors were greatly smaller in amount. As this simple artifice is not, I believe, generally adopted, and as it seems greatly preferable to the employment of interpolation which must frequently be guessed at, and always open a door to the practice of admitting hypothetical numbers on the same footing with observed ones, I think it worth while to give a practical example of the mode I pursued, and the forms of my Tables. The normal hour employed was 10 P. M., at which time the observations appear to have been been most regularly made; and it is surprising how few observations I was obliged to discard for want of this standard of comparison. I have selected the Tables for June 1828, in which 18 observations out of 150 were wanting, the first reduction of which, by the method of simple means, gives the oscillation from 10 A. M. to 4 P. M. in the wrong direction, which is again restored by the application of the second method I employed, shewn on the opposite page.

JUNE 1828,—132 OBS.

Day.	8–8½ A. M.		10 A. M.		4 ± P. M.		8 P. M.		10 P. M.	
	Barom.	Ther.	Barom.	Ther.	Barom.	Ther.	Barom.	Ther.	Barom.	Ther.
	Inches.	°	Inches.	°	Inches.	°	Inches.	°	Inches.	°
1	27.394	67			29.454	70	29.440	65	29.394	67
2	29.200	67	29.224	68	29.290	63			29.374	64
3	29 400	64	29.400	65	29.216	65	29.162	65	29.096	65
4	29.058	67	29.020	70			28.810		28.844	64
5	28.956		28.986	67			29.084	64	29.114	63
6	29.168	64	29.180	64	29.326	64	29.384	62	29.428	62
7	29.628	60	29.620	65	29.658	64	29.676	64	29.700	62
8	29.764	63	29.768	64	29.784	66	29.808	62	29.830	64
9	29.830	65	29.818	66			29.780	65	29.794	64
10	29.810	67	29.814	68			29.750	62	29.728	64
11	29.780	67	29.774	68	29.770	64	29.762	62	29.794	64
12	29.818	66	29.832	68	29.834	66			29.870	64
13	29.856	67	29.828	66	29.864	65	29.884	65	29.894	63
14	29.950	67	29.956	65	29.948	62			29.946	57
15	29.898	60	29.894	60	29.820	60	29.800	60	29.798	59
16	29.710	60	29.700	61	29.642	63	29.562	62	29.612	62
17	29.484	60			29.330	60	29.296	59	29.280	61
18	29.222	61	29.286	65					29.300	62
19	29.340	64	29.352	65	29.384	65	29.404	64	29.434	63
20	29.478	66	29.470	67	29.450	65	29.430	64	29.444	63
21	29.428	65	29.428	67					29.378	64
22			29.484	68	29.530	68	29.582	66	29.606	66
23	29.732	64	29.750	64	29.784	65	29.796	65	29.818	64
24	29.886	68	29.888	70	29.870	70	29.894	68	29.900	67
25			29.834	71	29.800	68	29.802	67	29.808	67
26	29.872	70	29.876	70	29.872	69	29.856	68	29.850	67
27	29.850	70	29.850	70	29.800	71	29.794	68	29.794	66
28							29.750	68	29.750	66
29	29.730	66	29.734	66	29.704	66	29.716	64	29.728	64
30	29.700	64	29.698	66	29.616	68	29.592	62	29.590	64
Sum,....	798.942	1689	799.464	1794	681.746	1507	739.814	1541	887.896	1912
Mean,...	29.5904	65.0	29.6098	66.5	29.6411	65.5	29.5926	64.2	29.5965	63.7
Cistern Level,	+.0033		+.0039		+.0048		+.0034		+.0035	
Temp....	—.0974		—.1018		—.0990		—.0951		—.0937	
	29.4963		29.5119		29.5469		29.5009		29.5063	

The barometer employed at this time not being on the syphon construction, required a correction for the capacity of the cistern. The dashes in the columns of the following Table indicate a difference from the standard number equal to zero.

JUNE 1828,—132 OBS.

Day.	10 P. M.		8 — 8½ A. M.				10 A. M.				4 ± P. M.				8 P. M.			
	Barom.	Ther.	Barometer.		Ther.		Barometer.		Ther.		Barometer.		Ther.		Barometer.		Ther.	
	Inches.	°	+	—	+	—	+	—	+	—	+	—	+	—	+	—	+	—
1	29.394	67									.060		3		.046			2
2	29.374	64		.174	3			.150	4			.084		1				
3	29.096	65	.304			1	.304				.120				.066			
4	28.844	64	.214		3		.176		6							.034		
5	29.114	63		.158				.128	4							.030	1	
6	29.428	62		.260	2			.248	2			.102	2			.044		
7	29.700	62		.072		2		.080	3			.042	2			.024	2	
8	29.830	64		.066		1		.062				.046	2			.022		2
9	29.794	64	.036		1		.024		2							.014	1	
10	29.728	64	.082		3		.086		4						.022			2
11	29 794	64		.014	3			.020	4			.024				.032		2
12	29.870	64		.052	2			.038	4			.036	2					
13	29.894	63		.038	4			.066	3			.030	2			.010	2	
14	29.946	57	.004		10		.010		8		.002		5					
15	29.798	59	.100		1		.096		1		.022		1		.002		1	
16	29.612	62	.098			2	.038			1	.030		1			.050		
17	29.280	61	.204			1					.050			1	.016			2
18	29 300	62		.078		1		.014	3									
19	29.434	63		.094	1			.082	2			.050	2			.030	1	
20	29.444	63	.034		3		.026		4		.006		2			.014	1	
21	29.378	64	.050		1		.050		3									
22	29.606	66						.122	2			.076	2			.024		
23	29.818	64		.086				.068				.034	1			.022	1	
24	29.900	67		.014	1			.012	3			.030	3			.006	1	
25	29.808	67					.026		4			.008	1			.006		
26	29.850	67	.022		3		.026		3		.022		2		.006		1	
27	29.794	66	.056		4		.056		4		.006		5				2	
28	29.750	66															2	
29	29.728	64	.002		2		.006		2			.024	2			.012		
30	29.590	64	.110				.108		2		.026		4		.002			2
Sum,....	887.896	1912	+.210		+39		—.008		+76		—.242		+42		—.214		+4	
Mean,...	29.5965	63.7	+.0078		+1.5		—.0002		+2.8		—.0105		+1.8		—.0086		+1.6	
Cistern Level,	+.0035		+.0002				.0000				—.0003				—.0002			
Temp....	—.0937		—.0044				—.0082				—.0053				—.0047			
	29.5063		+.0036				—.0084				—.0161				—.0135			
			29.5099		65.2		29.4979		60.9		29.4902		65.5		29.4928		65.3	

The general results of the monthly means fully justified my expectations. The extent of the deviations on both sides, from the mean oscillation given by the monthly results, was greatly diminished, and likewise the number of times which the oscillation came out *negative*, or in the wrong direction.

6. By consulting Tables I, II, III, and IV, at the end of this Memoir, the partial means of each month during which observations were made will be found for the years 1827, 1828, 1829, and 1830. As there cannot be a doubt that the seasons produce a very sensible effect on the amount of the oscillation, I prefer deducing the general result from the mean of the different seasons, which are contained in the four following Tables, the absolute numbers being *restored* from the data of the previous ones. The following are the general means, the spring period being understood to include March, April, May; summer, June, July, August; autumn, September, October, November; and winter, December, January, February.

Seasons.	Number of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
Spring,	1159	29.3651	29 3589	29.3438	29.3618	29.3640
Summer,	977	29.3896	29.3820	29.3715	29.3812	29.3866
Autumn,	1107	29.4378	29.4422	29.4286	29.4360	29.4312
Winter,	1167	29.4386	29.4447	29.4416	29.4447	29.4425
Sum,	4410	117.6311	117.6278	117.5855	117.6237	117.6243
Mean,		29.4078	29.4070	29.3964	29.4059	29.4061

It thence appears that the principal oscillation from the first morning observation to the afternoon one, amounts to — .0114 inch; that from the afternoon to the second evening one, to + .0097 : the ratio is 1 : 1.18, which is considerably less than the best observations give for the south of Europe. In the north we have no data of comparison, since, as far as I know, no register capable of affording satisfactory results for the evening maximum has

ever been kept in any part of Britain *. In the comparisons I am about to institute with the observations made in other parts of the world, I shall adopt the oscillation between 10 and 4, or — .0106 inch, since this agrees nearly with the usual hours of observation, as it has rarely happened that meteorologists have taken the trouble to observe the barometer at two periods near one another. The amount just named exceeds somewhat the oscillation observed by SOMMER at Königsberg, in Latitude 54° 42′, which has hitherto been referred to as the solitary result from northern Europe †. We shall afterwards see that the amount I have assigned agrees much more closely with the strict analogy of other observations in more southern latitudes. Besides, the hour at which the afternoon minimum was observed by SOMMER was much too early in all probability, being part of the time at 2 P. M., and the remainder at 3 P. M. The hour in temperate climates is probably later than is generally assigned: nor does it add much to our confidence in the results, that BESSEL informs us, that "notwithstanding the minuteness of the amount, it is recognised in each *annual* mean ‡," when in my tables it is eliminated in 28 out of 38 *monthly* means, notwithstanding its extreme minuteness in winter. In London, where the amount is more than twice as great as here, and the observations are made only twice a-day, but with such regularity as to avoid the necessity of any artifice for correcting omissions, I find the oscillation detected in 46 monthly means out of 48 §; and at Paris, in every one of 132 months ‖, the amount there being nearly three times that at Edinburgh.

* I do not except Mr DANIELL's Observations for reasons assigned below, §. 9.

† HUMBOLDT, Relation Historique, 4to edition, tom. iii. p. 306.

‡ Astronomische Nachrichten, 1823, No. 26.

§ Philosophical Transactions for 1827, &c.

‖ See M. BOUVARD's Memoir in the Memoires de l'Institut for 1824.

7. The influence of the seasons upon the horary oscillation is an important inquiry; and considerable uncertainty prevails in the best observations. With regard to the *hour,* we find the duration both of the morning and evening oscillation is greater in spring and summer than in autumn and winter; 8 or half-past 8 A. M. and 10 P. M. being the critical hours at the former period, and 10 A. M. and 8 P. M. at the latter. With regard to the variable amount, if we take advantage of the double hours of observation, and select the actual maxima, we shall find the following amounts of oscillation:

Seasons.	Morning Period.		Evening Period.	
	Hour.	Oscill.	Hour.	Oscill.
Spring, ...	8	.0213	10	.0202
Summer, . . .	8	.0181	10	.0151
Autumn, ...	10	.0136	8	.0074
Winter, ...	10	.0031	8	.0031

We thus see that both the morning and evening tide exhibits the same order of decrease through the seasons, descending in winter, to a very minute amount.

If we employ the fixed hours of 10 A. M. and 10 P. M. for the maxima, we shall find the oscillation at a maximum in spring and autumn, lower in summer, and the decided minimum in winter. This agrees perfectly with the results of the observations made at the apartments of the Royal Society, in London, which I have collected from the Philosophical Transactions for the last four years, during which the meteorological register has been conducted on a much improved plan.

Year.	SPRING. Mar. Apr. May.	SUMMER. June, July, Aug.	AUTUMN. Sept. Oct. Nov.	WINTER. Dec. Jan. Feb.
	Inches.	Inches.	Inches.	Inches.
1827.	.013 .025 .024	.032 .021 .014	.025 .026 .021	.021 .026 .025
1828.	.027 .024 .036	.016 .008 .035	.079 .019 .024	.006 .010 .011
1829.	.031 .025 .027	.028 .023 +.005	.007 .006 .064	.012 .005 .014
1830*.	.035 .006 .012	.009 .013 .026	.011 .036 .059	.002 .047 +.001
Sum, Mean,	.285 .0237	.220 .0183	.377 .0314	.178 .0148

That these differences are really owing to the influence of the seasons may be inferred from the excellent agreement of the four total annual means, which are ·0227, ·0246, ·0205, ·0214. And we may reasonably conclude, by the analogy of my observations, that, had the true hours of tides been observed, the order in intensity would have been the same. The best observations in other parts of the globe, afford us, it must be admitted, somewhat anomalous results. In the excellent observations of Marqué Victor, at Toulouse, little or no difference from the influence of the seasons was observed †. At Clermont Ferrand the admirable observations of Ramond assure us that the maximum oscillation occurs in spring, and the minimum in winter ‡, with a secondary maximum and minimum in autumn and summer.

* For part of those for 1830, I am indebted to Mr Hudson, the observer, whose zeal in the examination of this phenomenon promises soon to afford us new and valuable data.

† Bibliotheque Universelle, xx. 246.

‡ Memoires de l'Institut, 1812.

At Paris, from eleven years' results, the maximum is also in spring, but the principal minimum in summer, and having a second maximum in autumn. On the Plateau of Bogota, near the Equator *, there are two maxima, which appear nearly to coincide with the maximum intensity of the solar rays, occurring towards the equinoxes. The observations of Dorta, at Rio Janeiro, in 23° S. Lat., give nearly the same results, the maximum being in April, or at the autumnal period in southern latitudes †. It is remarkable also, that the observations of Dr Russel at Berhampour (Lat. 24° N.), and of Mr Prinsep at Benares (Lat. 25½° N.), each continued for three years, give, severally, a minimum in summer, and one not so low in winter, with a decided maximum in spring, and a strong indication of a smaller one in autumn ‡. If, as I think we have good reason to believe, the real maximum is in spring, according to the majority of observations, we may account for this very general apparent maximum in autumn, by considering the effect of change in the hour of tide. For, suppose the barometer observed, as has very generally been the case, constantly at 9 a. m., this corresponds to the true critical hour very exactly at the equinoxes; but in summer, as this hour becomes earlier, the barometer will have passed its maximum at 9 a. m., and the oscillation will come out too small, giving an *apparent* minimum in summer, when perhaps none ought to exist. This, probably, will account satisfactorily for this extensively observed fact, and accords well with the results of my observations at double hours. If we can trust to the reductions of Marqué Victor's observations at Toulouse, they present the greatest anomaly in this part of the subject.

* Humboldt, Relation Historique, iii. p. 302

† Ibid. p. 298.

‡ Philosophical Transactions, 1828.

The monthly oscillations at the remarkable station of the Grand St Bernard will be noticed in Section 11.

8. Without dwelling farther at present upon the deductions from my own observations, I proceed to make some inquiry into the law which regulates the geographical distribution of this remarkable phenomenon.

The general fact that the amount of the barometric oscillation decreases from the Equator towards the Poles, has been long established. The active researches of HUMBOLDT, between the tropics, above thirty years ago, excited a spirit of inquiry in Europe, in which the distinguished RAMOND took the lead, and which terminated in the detection of the horary oscillation among the great accidental variations of atmospherical pressure, at the same hours nearly, but much smaller in amount, than in lower latitudes. The labour of observation was therefore greatly increased, requiring some years of accurate data to afford the desirable precision as to amount, which might be obtained by a few days observation near the Equator. Allowing for accidental irregularities, the progressive decrease has been very satisfactorily proved; but two very remarkable registers have, within the last few years, furnished us with unexpected and curious results, which must occupy a prominent place in any theory on the subject. The first of these was the register kept at the convent of the Grand St Bernard, which, as well as a precisely corresponding one at Geneva, was adapted to the investigation of the horary oscillations, immediately after the publication of the third volume of HUMBOLDT's "Relation Historique." The St Bernard observations demonstrate, by the regular annual results of the last five years (1826–30), that, at 8000 feet above the sea, the barometer is *lowest* at 9 A. M. and *highest* at 3 P. M., precisely the reverse of what had hitherto been observed. HUMBOLDT had remarked that, between the tropics, though the amount was diminished, the hour of maximum was not changed. These two

results have sometimes been much misunderstood, and spoken of as if they stood in opposition to each other. Mr Daniell (who is almost the only English writer by whom this subject has been considered at any length), treats the question as one of *quality* not of *degree*. The atmospheric tide is much smaller in Europe, at the level of the sea, than at the same level at the Equator: if, therefore, we consider the influence of height to be equal in both cases, and in the same direction, it is very obvious that while, at the Equator, the oscillation may be only diminished in dimension, at the same height in the Temperate Zone, it may have become null, or changed its sign; that is, being *negative*, the time of maximum below will correspond with that of minimum above, and *vice versa*,—a phenomenon perfectly in conformity with the laws which ordinarily regulate physical causes, and deducible from the abstract consideration of quantity, therefore to be considered in no respect as an anomaly in the *quality* of the effect, or as a change *per saltum* in the course of nature.

9. But, what is very remarkable, a precisely analogous fact has been discovered in Lat. 74°, by Captain Sir Edward Parry, whose admirable meteorological registers were a most valuable donation to science. This circumstance had for some years been suspected, from the earlier journals of Parry and other Arctic voyagers; but these results were received with just doubt, because the barometric registers wanted the indispensable element of the temperature of the mercury, which, under any circumstances, and especially those of an arctic voyage, might produce, by an average difference of temperature at one hour of the day from that at another, results, erroneous in amount, or even opposed to the truth. For example, it will be seen by the four first tables attached to this paper, that in the entire annual results, even in this temperate climate, a constant excess of temperature at 4 p. m. was observed above that at 8 a. m., amounting to a degree and a half of Fahrenheit, which, supposing no oscillation, would give rise to a *negative* one of between four and five thousandths of an

inch, or nearly half the observed amount in this latitude. It is, therefore, not wonderful that the first observations, which wanted the attached thermometer, should have been received with distrust, and it is rather surprising that Mr DANIELL should have so overlooked this source of error, as to have placed the utmost confidence (in the first edition of his work) in the existence of oscillations which might have been caused by a change of temperature amounting to little more than 1° Fahrenheit; and that he should have published his deduction for the horary oscillation at London from his own observations, which wholly wanted this element. The last voyage of Captain Sir EDWARD PARRY afforded results worthy of the highest confidence, from observations with excellent instruments, of which the indications were registered with an assiduity and precision which puts to the blush anything of the kind, at least in Britain, destined to the furtherance of the science of meteorology. These excellent observations indicate the existence of all the oscillations observed in lower latitudes, including that at 4 A. M., which has rarely been observed in any part of the globe, and give all the values with a negative sign, relatively to the ordinary oscillations. The six monthly means (Nov. 1824—April 1825) give *every one* the negative tide from 4 A. M. to 10 A. M., and likewise from 10 A. M. to 4 P. M., and from 10 P. M. to 4 A. M.; and every month but one from 4 P. M. to 10 P. M. As the greatest observed tide was that from 4 A. M. to 10 A. M., or —.0089 inch, I shall adopt it in future computations, more especially as from three months' observations, when the daily number was extended to twelve, the critical hour in the afternoon appeared to be later than 4, perhaps considerably*.

* A similar fact with regard to Northern Europe has been suggested by some observations made at the apartments of the Royal Society of London, in a paper by Mr LUBBOCK, V.P.R.S. in the Phil. Trans. for 1831, of which the author has been kind enough to favour me with a copy since this paper was read.

These observations, then, establish the existence of an oscillation in Lat. 74°, nearly equal to that in Lat. 56°, and affected by an opposite sign.

10. The number of observations in different parts of the globe being now very considerable, it appeared to me of high importance, in the present state of the science of meteorology, to endeavour to generalize them, and see how far they might be represented by an empirical law. I was accordingly engaged in classifying observations collected from every quarter, when I was fortunate enough to meet with an abstract of a very important memoir, by M. Bouvard, of the observatory of Paris, upon this subject, and read recently at one of the annual meetings of the Helvetic Society. It is upon the hourly variations of the barometer; and the only abstract of it I have met with is that contained in the *Bibliotheque Universelle* for 1829, nor have I seen it in any other periodical work. M. Bouvard, whose important contributions to meteorology are universally known, has here amassed a great addition to the observations collected by Humboldt; and he undertakes the bold enterprise of representing the extent of the oscillation in any latitude, at any height above the sea, and at any period of the day or year, by an arbitrary formula: but though his table presents perhaps more accordance with observation than might have been expected from so sweeping a generalization, I think there is much reason to question the accuracy of the formula, which is founded on these conditions;—that at the equator the extent of the oscillation is proportional simply to the temperature, on the centigrade scale, of the period during which the oscillation is observed at the given spot, the oscillation and temperature at the level of the sea being unity;—that in any other latitude, the same law is to be modified by introducing the additional proportionality to the square of the cosine of the latitude. Or, representing by m and t the tide and mean temperature at

any place and for any period, in latitude θ; and by m' and t' those quantities at the equator, M. Bouvard gives

$$m' = \frac{t'}{t} \frac{m}{\cos.^{2}\theta}$$

By this the column of calculated results in the table I now subjoin has been computed, the temperatures in which, it is to be observed, are not the mean temperatures of the place, but of the hours included in the great period from 9 or 10 A.M. till 3 or 4. P.M. I must also remark, that where the number of daily observations was considerable, M. Bouvard has made use of a formula depending on the sine of the diurnal arc. The application of this formula is very briefly noticed in the abstract to which I allude; but it appears to have been almost entirely confined to intertropical observations, where the oscillation has frequently to be eliminated from the irregular observations of a few days. In northern latitudes I have generally found the amount unaltered from the original announcement in the various publications to which these scattered results have been confined. I have added to this valuable table, which contains many results previously unpublished, a few remarks as they occurred to me.

TABLE of the First Period of the Hourly Variations of the Barometer, reduced to the Equator at the Level of the Sea, by M. Bouvard's Formula.—Equatorial Temp. = 30° Cent.

OBSERVERS.	LOCALITIES.	Height in Metres.	Latitude.	Temp. Centigr.	Oscillations observed.	Reduced to Equator.	REFERENCES.
			° '	°	Millim.	Millim.	
Duperrey.	Offak, 3 days observation, . .	10	0 2	28,0	2,93	3,14	From MS. obs.
Freycinet.	Rawak, 4 days, ...	10	0 2	26,0	2,61	3,01	Ditto.
La Condamine.	Quito, doubtful observations, .	2907	0 14	16,5	2,25	4,09	Humboldt.
Humboldt.	Antisana, a single day, weather unfavour.	4093	0 33	8,5	0,97	3,42	Ditto.
Caldas.	Popayan, 4 days, ...	1776	2 26	18,0	2,07	3,45	Ditto.
Jacquin Purera.	St Louis Maranhan, a year, Engl. Bar. without thermometrical correct. (a),	10	2 30	29,0	3,79	3,93	Orig.
Humboldt.	Ibague, 3 days, ...	1370	4 28	21,0	2,59	3,72	Humboldt.
Caldas.	Santa Fé de Bogota, 19 successive months,	2660	4 36	17,0	2,10	3,73	Ditto.
Boussingault.	Idem, by more than 1000 observations,	—	—	17,0	2,30	4,09	Ditto.
Duperrey.	Payta, 5 days (b), ...	10	5 6	27,0	2,66	2,98	MS. obs.
Idem.	Ascension (Isle) 3 days, 6 A.M. to 6 P.M.	10	7 55	28,0	2,43	2,72	MS. obs.
Freycinet.	Coupang, 9 days, ...	10	10 9	28,0	2,96	3,27	MS. obs.
Humboldt.	Cumana, 12 days, ...	10	10 28	26,0	2,55	3,05	Humboldt.
Idem.	Caracas, 11 days, ...	936	10 31	21,0	2,70	3,99	Ditto.
Idem.	Guayra, 11 days, ...	10	10 36	26,5	2,75	3,22	Ditto.
Idem.	Lima, 4 days, ...	166	12 3	23,0	2,77	3,77	Ditto.
Idem.	Callao, 3 days, ...	10	12 3	20,0	2,22	3,48	Ditto.
Simonoff.	O-Taïti, 7 days, ...	10	17 29	24,0	2,15	2,95	Zach, Corr. Astr.
Humboldt.	Mexico, 3 days, ...	2231	19 26	17,0	1,80	3,57	Humboldt.
Freycinet.	Port Louis, Isle of France, 20 days,	10	20 10	22,0	1,72	2,66	MS. obs.
Duperrey.	Idem, 4 days, ...	—	—	24,0	1,92	2,73	MS. obs.
Freycinet.	Rio Janeiro, 2 days, ...	10	22 54	24,2	2,58	3,77	MS. obs.
Dorta.	Idem, 12 months, ...	—	—	—	2,33	3,39	Humboldt.
Russell.	Berhampour (India), 3 years, .	......	24 4	24,0	2,23	3,42	Phil. Trans.
Prinsep.	Benares, 3 years, ...	......	25 30	26,0	2,69	3,81	Ditto.
Coutelle.	Cairo, 25 days, ...	......	30 3	22,0	1,89 (c)	3,44	Voyage d'Egypte.
Freycinet.	Port Jackson, 19 days, ...	10	33 51	21,0	1,71	3,52	MS. obs.
Gambart.	Marseilles, 5 years, ...	46	43 17	16,0	0,83	2,94	Conn. des Temps.
Marqué Victor.	Toulouse, 4 years, ...	156	43 36	15,0	1,00 (d)	3,81	Acad. Toulouse.
Valz.	Nismes, 1 year, ...	65	43 50	16,5	0,98	3,42	Orig.
D'Hombres Firmas.	Alais, 3 years, ...	132	44 7	16,5	0,99	3,49	Ditto.
Gasparin.	Orange, 5 years, ...	......	44 8	16,5	0,85	3,00	Ditto.
Billiet.	Chambery, 18 months, ...	267	45 34	14,0	1,00	4,37	Acad. Chambery.
	St Bernard, 5 years (e), . . .	2491	45 42	—0,7	+0,046	4,04	Bibl. Univ.
Ramond.	Clermond-Ferrand, 7 years, . .	410	45 46	12,5	0,94	4,53	Mem. de l'Instit.
	Geneva, 3 years, ...	407	46 12	12,8	0,74 (f)	3,62	Bibl. Univ.
	Bevers (Switzerland), ...	1657	46 34	7,5	0,45	3,81	Orig.
Fueter.	Berne, 10 years, ...	532	46 57	12,0	0,90	4,82	Bibl. Univ.
Horner.	Zurich, 1 year, ...	405	47 22	13,0	0,88	4,43	Ditto.
	St Gall, 1 year, ...	635	47 26	11,0	0,56	3,34	Ditto.
Herrenschneider.	Strasburg, 12 years, ...	150	48 35	13,0	0,88	4,29	Orig.
Bouvard aîné.	Paris, 12 years, ...	65	48 50	13,8	0,76	3,81	Mem. de l'Instit.
Nell de Bréauté.	La Chapelle, 8 years, ...	154	49 49	11,5	0,49	3,07	Orig.
Crahay.	Maëstricht, 9 years, ...	52	50 51	13,0	0,57	3,30	Quetelet.
Royal Society.	London, 1 year (g), ...	30	51 29	12,6	0,57	3,50	Phil. Trans.
Sommer.	Königsberg, 8 years, ...	30	54 42	6.0	0,20	3,27	Humboldt.

(a) On account of the want of an attached thermometer, this series ought to be discarded.

(b) Modified from the result given by Humboldt, Rel. Hist. iii. 312.

(c) This amount, somewhat different from that given by Humboldt, appears to be the result of a new analysis of the original observations in the "Voyage d'Egypte."

(d) This is *smaller* than was originally assigned by Marqué Victor.—See § 13. *Note.*

(e) These are the first five of the years given in § 11, with the exception of 1821. By a more extended and systematic reduction from the Table there given, I find the amount to be + .009 lines = 0mm.020.

(f) There is probably some mistake in assigning 0.74 as the amount at Geneva. For the years 1826–8, to which M. Bouvard must probably have referred, it was 0mm.86; and the mean of five years, 1826–30, gives 0mm.81.

(g) It will be seen by § 7, that the mean of four years gave .0223 inch. = 0mm.566.

11. I have now some more particular observations to add upon the theoretical part of the Table. *1st*, I conceive that it is little to be expected, or perhaps desired, that one formula should represent the decrement both in latitude and height, and the change at different hours and seasons, which, even in the much better known facts of terrestrial temperature, has never been, nor satisfactorily could be, executed. I conceive that, in the purely physical bearings of the question, the existence of such a common law would be surprising, because such a coincidence must have been derived from the concurrence of independent agencies. *2d*, The observations at considerable heights, in different latitudes, are too few, too brief, and too inconsistent, to afford us any thing like satisfactory results. For example, the observations at Caracas, at a height of 3000 feet, give a greater amount of oscillation than those at Cumana, at the level of the sea, at the same latitude. *3d*, M. BOUVARD proposes his formula as capable of expressing the variation observed at any period of the day or year, and has shewn that the ratio of the different mean temperatures of the periods of forenoon and evening oscillations, correspond tolerably nearly with that of the observed amount of these oscillations, at least in Europe; but I would ask, Why do not the great differences of temperature, corresponding to the seasons, give the marked results which his formula would suppose, producing a tropical amount in summer, and an arctic one in winter? At Paris, for instance, where the mean of the summer temperatures, that is, of the months of June, July, and August, is, according to HUMBOLDT *, 18°.1 Cent.; and of the winter months (December, January, and February), 3°.7 Cent., the ratio is 4.9 : 1; whilst the corresponding oscillations, which, according to M. BOUVARD's formula, ought to have the

* Des Lignes Isothermes. Mem. d'Arceuil, tom. iii. *Table*.

same ratio, are $\overset{\text{m.m.}}{0.766}$ and $\overset{\text{m.m.}}{0.685}$, or as 1.1 : 1,—this ratio being less than *one-fourth* of the other. Yet the Paris observations for the barometric tide are among the best we possess; and these numbers are a mean from the registers of eleven years. Again, at the very important station of the Grand St Bernard (to which M. BOUVARD strongly appeals in support of his formula), where the mean result is negative with regard to the oscillation at the level of the sea, the mean temperature of the year being below 0° cent., therefore rightly expressed in the preceding Table, we should have a very striking difference of result in summer and winter. The temperature of the three summer months being about + 6° cent., and of the three winter months about — 6° cent., the first should give a considerable *positive* oscillation, the latter a considerable *negative* one. Let us see, therefore, how the case stands. For this purpose, I shall classify under the seasons the whole of the monthly results which have been published, and of which the last five years are complete. The sign of + in the following Table indicates a rise between 9 and 3, that of — a fall: the former, therefore, is to be considered *negative* with regard to the ordinary course, since the barometer rises when it usually falls at the level of the sea. I may add, that, by taking the mean temperature of the day in place of that of the particular period in computing the oscillation by M. BOUVARD's formula, here and at Paris, we are doing it no injustice, as, though the summer and winter temperature would both be somewhat higher *, the *ratio* would be almost the same; and it is about no evanescent differences that we are disputing.

* Indeed there seems much reason to doubt whether the period of the day alluded to has a mean temperature below 0° cent. If it has not, M. BOUVARD's formula would totally fail, as the equatorial oscillation would come out with a wrong sign.

3

CONVENT OF THE GRAND ST BERNARD.

Year.	SPRING. March, April, May.	SUMMER. June, July, August.	AUTUMN. Sept. Oct. Nov.	WINTER. Dec. Jan. Feb.
	Lines.	Lines.	Lines.	Lines.
1821,				— 0.14
			— 0.03	
			— 0.09	
1822,	+ 0.04			— 0.15
	+ 0.06	— 0.01		— 0.12
	— 0.08	+ 0.11	— 0.06	— 0.15
1823,	— 0.05			
	+ 0.09			— 0.09
	+ 0.11			— 0.07
1826,	— 0.01	+ 0.03	— 0.38	— 0.04
	+ 0.07	+ 0.35	0.00	— 0.07
	+ 0.05	+ 0.11	+ 0.13	+ 0.05
1827,	— 0.02	+ 0.08	— 0.05	— 0.02
	+ 0.11	+ 0.01	— 0.08	+ 0.51
	+ 0.08	— 0.04	— 0.03	+ 0.06
1828,	+ 0.11	+ 0.07	— 0.01	— 0.03
	+ 0.06	+ 0.19	+ 0.03	+ 0.05
	— 0.02	+ 0.06	+ 0.12	— 0.01
1829,	+ 0.11	+ 0.07	— 0.01	— 0.03
	+ 0.06	+ 0.19	+ 0.03	+ 0.05
	— 0.02	+ 0.06	+ 0.12	— 0.01
1830,	— 0.36	+ 0.05	— 0.03	— 0.14
	— 0.12	+ 0.04	— 0.07	— 0.22
	+ 0.09	— 0.01	— 0.18	— 0.03
Sum,	+ 0.38	+ 1.36	— 0.59	— 0.60
Mean,	+ 0.018	+ 0.080	— 0.033	— 0.029

It can hardly require a word from me to point out how well the individual results justify the deductions of the mean, so that there are only *three* — results in the summer period, and only *five* + in the winter one; therefore, instead of an oscillation corresponding to the Temperate Zone in summer, and to the

Arctic Regions in winter, we have precisely the reverse. It is hardly necessary to remark, how totally opposed this is to the theory of M. Bouvard*.

12. I would remark, *fourthly*, upon M. Bouvard's formula, that, by fixing the mean temperature from which he calculates his numerical results at the mean of the period from 9 A. M. to 3 P. M., he has given himself certain limits of which it is almost impossible for a speculator not to avail himself, and which must be at all times unsatisfactory on account of the great want of the thermometrical observations for fixing the mean temperature of any particular portion of the day in so many latitudes. I have to observe, in the *last* place, that the results are far from satisfactory even when the difficulties of the subject are considered. By consulting the last column of the table already given, we find the deduction of the formula for the oscillation at the Equator range from 2.72 to 4.82 millimetres; and, what is worse, that the + and — errors from the mean are not well balanced throughout the quadrant of latitude, but that there is a general rise in their value as we proceed farther from the Equator. As it is undeniable that the tropical observations, from the smallness of the reductions required, afford the most correct results, I am strongly of opinion that the mean of his numbers would give us an equatorial oscillation greatly above the truth. By looking over M. Bouvard's table, we shall find not a single authentic series of observations gives an oscillation of three millimetres (we except the observations at St Louis Maranhan, which, wanting the results of the attached thermometer, should be discarded), the maximum being 2.93, which occurred at the very Equator. Humboldt considers 3.3 millimetres the extreme limit

* In 1829, the mean temperature of the seasons, distributed as above, was — 1°.5 R. + 4°.8 R. + 0°.5 R., and — 4°.6 R. In 1830, it was colder, as follows: — 0°.1 R. + 4°.5 R., — 0°.7 R., and — 7°.7 R. respectively.

for the Equator; and I believe there is no series in existence, continued for several days together, which gives such a mean result. The observations of Duperrey, at Payta, which, by Humboldt's table, was reckoned at so extreme an amount as 3.4, has dwindled, by the analysis of M. Bouvard, to 2.66, which is, in all probability, a very correct result; and my own opinion is, that future observations will limit the equatorial oscillation to 3 millimetres, or perhaps even lower, except in anomalous situations. Now, the result of the table we are discussing, generally gives an oscillation nearer 4 than 3 millimetres; but what is remarkable, the larger numbers occur only where the amount of reduction is considerable. Thus, confining ourselves to observations not at extraordinary heights above the level of the sea (that is, under 2000 metres), we do not find a single observation under latitude 45°, affording an equatorial oscillation amounting to 4 millimetres; whilst between 45° and 55°, in less than half the number of localities, we have no less than five results above 4 millimetres. This alone indicates something wrong. Had M. Bouvard limited his passion for generalization to the law of *latitude* only, he would easily have given a nearer approximation; this was what I had proposed to undertake before I had the good fortune to meet with his paper, and what I afterwards executed with the aid of his data.

13. As M. Bouvard has presented us in his table with a considerable number of unpublished results, which I was unwilling to lose, I have trusted to the skill and fidelity of the author in deducing the numbers which he has given as the *observed amount* in his table. From observations continued but a few days, and at irregular hours, it requires some labour and judgment to obtain the truest results. It is therefore almost entirely in equatorial results that M. Bouvard differs in his numbers from the quantities already assigned by such observers as have published their registers, and from a very careful and extended comparison which I have made with the originally published synopses of observations

made in the Temperate Zone, I rarely find any change made upon them *. The table given in Sect. 10 has therefore been my guide, as far as absolutely observed results are there given; and it would be unjust not to add here, that M. Bouvard deserves the thanks of all who take an interest in the subject, for the spirited and valuable paper from which the table is extracted, and for the unpublished or scattered results which he has there combined, as well as that his particular theory is proposed with much diffidence.

As my investigation extends merely to the law of diminution connected with latitude, I have confined myself to observations near the level of the sea. The greatest exception is the result of M. Ramond at Clermont-Ferrand, at the height of 410 metres; but the excellence of the observations is such, and they agree so well with those in the same parallel, that I could not omit them. I have added the value given by Captain Sir Edward Parry's observations for Port Bowen, and my own for the neighbourhood of Edinburgh. My object being to determine the law of variation with latitude which should best represent a given number of trust-worthy observations of the horary oscillation, I have employed a mode of investigation similar to that used by Mr Atkinson, in his analysis of the law of mean temperature, forming

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* The principal deviation which I have remarked is in the extensive series of M. Marqué Victor, at Toulouse, where the observations were made many times a-day, and probably had M. Bouvard's formula of reduction for hours applied to it. However, it seems rather unaccountable how any formula of this kind should *diminish* instead of increase the amount of oscillation, which it actually does in this case, unless there be some numerical mistake, the amount derived by M. Marqué Victor (and which I have verified as far as the combination of the annual means is concerned) being $1^{mm}.20$; according to M. Bouvard it is $1^{mm}.00$. As, however, M. Bouvard treats this as the best determined point in Europe, it is probable that he had carefully examined the tables in the *Memoires de l'Academie de Toulouse*, the later volumes of which I have not been fortunate enough to see.

part of his elaborate paper upon Astronomical Refractions, inserted in the second volume of the Memoirs of the Astronomical Society.

14. I assumed the form of the function of the latitude which expresses the oscillation, to be some unknown power of the cosine, affected by an unknown coefficient, and constant. These quantities are therefore to be eliminated from three or more equations, derived from observation in different parts of the globe. Let the barometric oscillation be represented by $z_{,}, z_{,,}, z_{,,,}$ in latitudes $\theta_{,}, \theta_{,,}, \theta_{,,,}$ respectively: then, denoting by n the unknown power of the cosine, and by α and ζ the other constants, we may separate the first of these unknown quantities from three equations of the form $z = \alpha \cos^n \theta + \zeta$, by putting them under that of $\frac{z_{,} - z_{,,,}}{z_{,} - z_{,,}} = \frac{\cos^n \theta_{,} - \cos^n \theta_{,,,}}{\cos^n \theta_{,} - \cos^n \theta_{,,}}$. Whence the value of n may be deduced by approximation. For this purpose, I selected nine values of z from the list of observed oscillations, and, for greater convenience of calculation and tabulation, I shall employ these during the rest of this paper in the form of *millimetres*, which in fact, since Humboldt wrote, have become the most common units of measure for these results. These numbers were not in general single results, but were combined from several in such a way as appeared best to correct accidental irregularities. Three observations nearly equatorial were taken, three towards the tropics, and three in high latitudes, as follow:

	Lat.	Oscill.		Lat.	Oscill.		Lat.	Oscill.
	° ′	m.m.		° ′	m.m.		° ′	m.m.
Ascension,	7 55 S.	2.43	Cumana,	10 28 N.	2.55	Lima,	12 3 S.	2.77
Coupang,	10 9 S.	2.96	La Guayra,	10 36 N.	2.75	Callao,	12 3 S.	2.22
Mean,	9 2	2.70	Mean,	10 33	2.65	Mean,	12 3	2.49

	Lat.	Oscill.		Lat.	Oscill.		Lat.	Oscill.
	° ′	m.m.		° ′	m.m.		° ′	m.m.
Isle of France,	20 10 S.	1.72, 1.92	Cairo,	30 3 N.	1.89	Port Jackson,	33 51 S.	1.71
Rio Janeiro,	22 54 S.	2.58, 2.73						
Mean,	21 22	2.14						

	N. Lat.	Oscill.		N. Lat.	Oscill.		Lat.	Oscill.
	° ′	m.m.		° ′	m.m.		° ′	
Marseilles,	43 17	0.83	Maëstricht,	50 51	0.57	Port Bowen,	73 48	—0.22
Toulouse,	43 36	1.00	London,	51 29	0.57			
Strasbrug,	48 35	0.83	Königsberg,	54 42	0.20			
Paris, .	48 50	0.76	Edinburgh,	55 55	0.27			
Mean,	46 4	0.86	Mean,	53 14	0.40			

Selecting nine of the possible combinations by threes, we obtain the following values for n:

Lat. 9° 2′, 21 22, 46 4	$n = 5.0$	Lat. 10° 33′, 30 3, 53 14	$n = 1.6$
Lat. 12° 3′, 33 51, 73 48	$n = 1.8$	Lat. 9 2, 38 3, 53 14	$n = 1.8$
Lat. 10 33, 33 51, 73 48	$n = 2.2$	Lat. 12 3, 21 22, 46 4	$n = 3.1$
Lat. 9 2, 33 51, 73 48	$n = 2.2$	Lat. 10 33, 21 22, 46 4	$n = 4.9$
Lat. 12 3, 30 3, 53 14	$n = 0.8$		

Mean value of n, $= 2.60$, or about $\frac{5}{2}$.

15. In order to modify the co-efficient and constant in the arbitrary formula, so as best to agree with observation, we shall select a series of observations from M. BOUVARD's Table, which, from their intrinsic merit, or from their representing accurately a group of minor observations, shall seem best fitted to give accurate results. We shall first take an approximate value of α and β from the observations at Cumana and Toulouse, which represent pretty nearly the whole of the rest. From them we obtain the two following equations:

$$2.55 = \alpha \cos^{\frac{5}{2}} 10^\circ 28' + \beta = .959\,\alpha + \beta,$$

$$1.00 = \alpha \cos^{\frac{5}{2}} 43 \quad 36 + \beta = .446\,\alpha + \beta.$$

$$\text{Whence } \alpha = 3.02\,;\ \beta = -0.35.$$

Substituting these values in the expressions for the selected oscillations, we obtain the following approximations:

PLACE.	Latitude.	Oscillation.		Error.
		Observed.	Approx. Calc.	
	° ′	Millimetres.	Millimetres.	Millimetres.
Payta, ...	5 6 S.	2.66	2.64	— 0.02
Cumana, ...	10 28	2.55	2.55	0.00
Otaheité, ...	17 29 S.	2.15	2.33	+ 0.18
Isle of France, . .	20 10 S.	1.82	2.23	+ 0.41
Rio Janeiro, . . .	22 54 S.	2.45	2.11	— 0.34
Cairo, ...	30 3	1.89	1.76	— 0.13
Toulouse, . . .	43 36	1.00	1.00	0.00
Clermont, . . .	45 46	0.94	0.88	— 0.06
Paris, ...	48 50	0.76	0.71	— 0.05
Maestricht, . . .	50 51	0.57	0.61	+ 0.04
Königsberg, . . .	54 42	0.20	0.42	+ 0.22
Edinburgh, . . .	55 55	0.27	0.36	+ 0.09
Port Bowen, . .	73 48	— 0.22	— 0.23	— 0.01

It is well worthy of remark upon the preceding Table, that, by means of the constants derived solely from the employment of observations in Latitudes 10° 8′ and 43° 36′, which have both a sign of +, we obtain a result for Lat. 73° 48′ N. not merely giving a sign of —, but agreeing in amount with singular precision with that which observation affords, proving that a *negative* oscillation, instead of being in any way anomalous, is nothing else than we might have inferred *à priori* for high latitudes.

16. In order that the constants may express the general result of all these thirteen observations in the best possible manner, we must reduce the sum of the squares of the errors to a minimum. Let $\alpha + \alpha'$ and $\beta + \beta'$ be the new values of the constants upon this supposition; let also e be the error of the above table, and E the reduced error. We then have

$$(\alpha + \alpha') \cos^{\frac{5}{2}} \theta + (\beta + \beta') - x = \mathrm{E},$$

and therefore,

$$\alpha' \cos^{\frac{5}{2}} \theta + \beta' + e = \mathrm{E},$$

which gives the following thirteen equations of condition:

$$.990\,\alpha' + \beta' - .02 = E_{I}$$
$$.959\,\alpha' + \beta' = E_{II}$$
$$.889\,\alpha' + \beta' + .18 = E_{III}$$
$$.854\,\alpha' + \beta' + .41 = E_{IV}$$
$$.814\,\alpha' + \beta' - .34 = E_{V}$$
$$.697\,\alpha' + \beta' - .13 = E_{VI}$$
$$.446\,\alpha' + \beta' = E_{VII}$$
$$.407\,\alpha' + \beta' - .06 = E_{VIII}$$
$$.352\,\alpha' + \beta' - .05 = E_{IX}$$
$$.317\,\alpha' + \beta' + .04 = E_{X}$$
$$.254\,\alpha' + \beta' + .22 = E_{XI}$$
$$.235\,\alpha' + \beta' + .09 = E_{XII}$$
$$.041\,\alpha' + \beta' - .01 = E_{XIII}$$

From which, proceeding by Legendre's method of minimum squares, we shall deduce $\alpha' = .0106$ and $\beta' = -.0254$, which, being substituted in the general equation, together with the values of α and β found above, it becomes

$$z = 3.031 \cos^{\frac{5}{2}} \phi - .381 \quad \text{for millimetres,}$$

$$= .1193 \cos^{\frac{5}{2}} \phi - .0150 \quad \text{for English inches.}$$

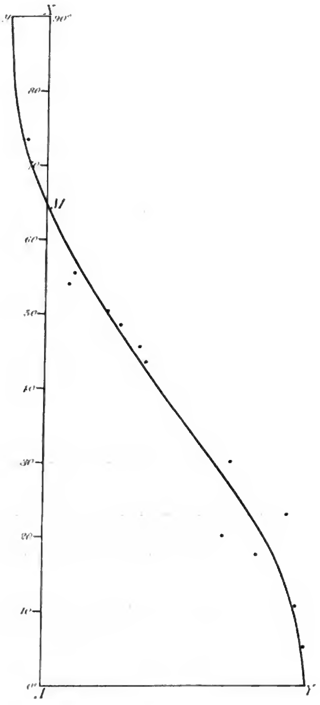
The formula for millimetres being applied to the observations already discussed, we obtain the following, as the *nearest possible* results:

PLACE.	Latitude.	Oscillation.		Difference.
		Observed.	Calculated.	
	° ′	Millimetres.	Millimetres.	Millimetres.
Payta, ...	5 6 S.	2.66	2.620	—.04
Cumana, ...	10 28	2.55	2.525	—.02
Otaheité, ...	17 29 S.	2.15	2.312	+.16
Isle of France, . .	20 10 S.	1.82	2.206	+.39
Rio Janeiro, . . .	22 54 S.	2.45	2.087	—.36
Cairo, ...	30 3	1.89	1.732	—.16
Toulouse, . . .	43 36	1.00	0.971	—.03
Clermont, . . .	45 46	0.94	0.851	—.09
Paris, ...	48 50	0.76	0.685	—.07
Maestricht, . . .	50 51	0.57	0.579	+.01
Königsberg, . . .	54 42	0.20	0.388	+.19
Edinburgh, . . .	55 55	0.27	0.332	+.06
Port Bowen, . .	73 48	—0.22	—0.256	—.04

By referring to Plate VII., the observed oscillations will be found projected by means of round dots, the distances of which from the line AX representing the quadrant of latitude, indicate the

PLATE VII.

Latitude	Oscillations observed in dim.
73°48′	0.22
55.55	0.27
54.12	0.20
50.51	0.57
48.50	0.76
45.46	0.94
43.36	1.00
30 3	1.89
22.54	2.45
20 10	1.82
17 29	2.15
10.28	2.55
5. 6	2.66



amount. The curve YMy, drawn through these points, AX being the axis of the abscissæ, AY that of the ordinates, represents the formula of greatest probability, just found. Since the oscillation becomes negative, there must be some point M where the curve crosses the axis, or where the oscillation is $=0$. This latitude, which may be denoted by L, is readily found by equating the value of z to zero, which gives

$$\cos L = \left(\frac{.381}{3.031}\right)^{\frac{2}{5}}, \text{ and } L = 64^\circ\ 8'\ 6''.$$

By putting $\theta = 0$ and $\theta = 90^\circ$, we obtain for the equatorial oscillation, or AY, $2^{mm}.650$; and for the polar, or Xy, $-0^{mm}.381$; or .119 and .015 English inches.

17. By means of the formula $z = \alpha \cos^n \theta + \beta$ (putting for α and β the corrected numbers), we may further deduce the mean amount of the atmospheric tide for the quadrantal arc of the meridian, and likewise corresponding to the entire surface of a hemisphere. In the first case, its mean value will correspond to the integral of $z d\theta$, taken between the limits $\theta = 0$ and $\theta = 90^\circ$, and divided by the length of the quadrant of latitude, or

$$\frac{2}{\pi}\int(\alpha \cos^n \theta + \beta)\, d\theta.$$

In the second case, we must introduce the expression for the length of the parallel of latitude, and divide the integral taken within the same limits as before, by the *surface* of the hemisphere, that is,

$$\frac{\int 2\pi\alpha \cos^{n+1}\theta d\theta + \int 2\pi\beta \cos\theta d\theta}{2\pi} = \int(\alpha \cos^{n+1}\theta + \beta \cos\theta)\, d\theta.$$

Since we have employed $n = \frac{5}{2}$, the integration of both these expressions comes under that of elliptic transcendentals; by a simple alteration, however, we shall obtain a direct solution, abundantly accurate for our present purpose. We have seen that $n = 2.6$; if, therefore, we make it $= 3$, instead of $\frac{5}{2}$, and mo-

dify α and β, we may obtain a different expression, which shall very closely represent the observations. We shall find for the new equation

$$z = 3.07 \cos^3 \theta - .36 \text{ as a near approximation.}$$

The first of the above integrals then becomes

$$\frac{2}{\pi}\int(3.07 \cos^3 \theta - .36)\, d\theta,$$

which, taken between the limits $\theta = 0$ and $\theta = \frac{1}{2}\pi$, gives for the mean oscillation, in relation to the quadrant of latitude,

$$\frac{4.09}{\pi} - .36 = \overset{\text{m.m.}}{0.94}.$$

The other integral above given becoming

$$\int(3.07 \cos^4 \theta - .36 \cos \theta)\, d\theta,$$

there results, within the same limits,

$$3.07 \cdot \frac{3}{16}\pi - .36 = \overset{\text{m.m.}}{1.45}$$

for the mean value with regard to the surface of a hemisphere.

These numbers correspond to homogeneous columns of air at the ordinary pressure and temperature 10½ and 16 metres in height respectively. In the investigation of any supposed connection with temperature, these mean results will be of some value.

18. I am satisfied for the present with having pointed out a formula representing very closely the observed amount of oscillation at the level of the sea, as depending upon latitude. For any successful generalization upon the influence of height, we must wait for vastly more extended data than we already possess; and the same remark is applicable, though to a less extent, to the influence of the seasons. That temperature has an important connection with the geographical distribution of this phenomenon, I have no doubt. It appears probable, that, among places under the same latitude, and having different

mean temperatures, the coldest has the smallest oscillation *. We know that in ascending above the level of the sea, they diminish together, the curve of temperature being probably asymptotic, whilst that representing the oscillation would appear to cross the axis at a certain height. At present, I have confined myself to arriving at a generality of the first degree; the higher degrees, which will embrace the element of temperature, will probably go far towards an explanation of the *cause*, with which that element is certainly nearly concerned. But we must be contented to wait in the mean time for additional data.

19. I have done perhaps as much as can be expected from a solitary observer towards fixing the minute quantity corresponding to this latitude. It is from public and learned bodies alone that we can look for registers of perfect regularity, combined with precision as to the hours of observation. I look with sanguine expectations towards the establishment of such a one by this Society. Strange though it be, I believe we may safely affirm, that Great Britain does not at this moment produce a register worthy of the present advanced state of meteorology. Scotland, by her geographical position, is well situated for unfolding many important phenomena of Nature, and, amidst the disadvantages of her inconstant sky, offers some peculiar recommendations to the zealous observer both in meteorology and magnetism; but of these it has been her misfortune to meet with few or none.

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* Thus Königsberg, though in a lower latitude, appears to have a smaller oscillation (see § 6.) than Edinburgh; but then the temperature appears to be only 43° F. (Astronomische Nachrichten, Feb. 1823), while that of Edinburgh is 47°. It must be hardly necessary here to observe, that, supposing the mean temperature of a place known in a function of the latitude, my formula admits of a direct com. comparison with the temperature (t) at the level of the sea. Thus, if Dr Brewster's formula of $t = 81.5 \cos \phi$ be employed, my formula becomes $z = a t^{\frac{5}{3}} - \zeta$; a being a new coefficient.

But the spirit must be fostered by her Societies. "La simultanéité et la durée," says an accomplished French philosopher, speaking of these bodies, "que leur institution donne à des efforts mortels, complètent la puissance de la méthode experimentale. Elles seules pouvaient desormais assurer la continuité du progrès des connaissances humaines; seules elles pouvaient développer les grandes théories, et faire obtenir des résultats qui, par leur difficulté, par la diversité, la persévérance, et l'étendue des travaux qu'ils exigent, n'auraient jamais été accessibles pour des individus *."

20. I cannot help remarking, in conclusion, that this observation was never more completely verified than in the hourly thermometrical observations made for some years together under the auspices of this Society, at the suggestion of its late learned Secretary Dr Brewster, which, I hesitate not to affirm, is the noblest donation ever made towards the progress of meteorological science †. Valuable, not merely from the specific results, important as they are, which it afforded, but even more so, as demonstrating the susceptibility of the science to assume a mathematical form, and proving that the confused obscurity which so long overhung the laws of mean temperature was due to the imperfections of our mode of observation, not to any anomaly in Nature itself, which experience daily more firmly convinces us is governed by laws equally immutable, whether palpable to the senses, or veiled by an indefinite series of secondary causes.

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* Biot, Notice des opérations enterprises pour déterminer la figure de la terre.

† The results are published in the Society's Transactions, Vol. X.

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21. Since this memoir was read, I have been favoured with a confirmation, particularly interesting, of the identity of the law which regulates the horary oscillation in the northern and the southern hemisphere. Captain PHILIP P. KING, R. N. permits me to make use of the results deduced from his admirable MS. register kept for six months together, at Port Famine, in the Straits of Magellan, South Lat. 53° 38′; West Long. 70° 54′. The following numbers give the difference of height in the barometer, reduced to 32° F. between 9 A. M. and 3 P. M. for each month *:

1828,	February,	—.016
	March,	—.008
	April,	—.026
	May,	—.026
	June,	—.006
	July,	—.042
	Mean Oscillation,	.0207
	Calculated by the Formula,	.0173
	Error,	—.0034

The temperature was so low (not exceeding 43° F. for the period from 9 A. M. to 3 P. M.) that M. BOUVARD's formula would err very greatly in defect.

* The abstracts of Captain KING's observations are about to be published in the proceedings of the Royal Geographical Society, vol. I.

TABLES.

N. B. ***The Barometric Numbers in the following Tables are all reduced to*** **32° F.**

TABLE I.—1827.

Month.	Number of Obs.	10 P. M.		8 A. M.		10 A. M.		4 P. M.		8 P. M.	
		Barometer.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.
		Inches.	°	Inches.	°	Inches.	°	Inches.	°	Inches.	°
Sept.	50	29.5767	63.0	—.0342	—0.4	—.0153	+4.5	—.0330	+1.2	—.0024	0.0
Oct.	138	29.3271	62.5	—.0050	—2.4	—.0072	—0.1	—.0175	—1.4	—.0006	+0.3
Nov.	145	29.5311	62.1	+.0358	—4.7	+.0325	—1.4	+.0339	—3.0	+.0080	—0.6
Dec.	129	29.0804	63.3	—.0040	—5.9	+.0049	—2.3	+.0009	—1.8	+.0029	+0.3
Sum,	462	117.5153	250.9	—.0074	—13.4	+.0149	+0.7	—.0157	—5.0	+.0079	0.0
Mean,		29.3788	62.7	—.0018	— 3.3	+.0037	+0.2	—.0039	—1.2	+.0020	0.0

TABLE II.—1828.

Month.	Number of Obs.	10 P. M.		8 A. M.		10 A. M.		4 P. M.		8 P. M.	
		Barometer.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.
		Inches.	°	Inches.	°	Inches.	°	Inches.	°	Inches.	°
Jan.	151	29.4211	55.2	—.0234	—2.5	—.0135	—1.2	—.0076	—2.8	+.0026	—0.4
Feb.	142	29.2914	56.0	+.0148	—2.8	—.0099	—1.7	—.0107	—3.6	+.0008	—0.5
Mar.	152	29.3473	57.5	+.0098	—3.1	+.0038	—1.9	—.0178	—2.1	+.0066	—0.3
April,	143	29.1989	60.0	—.0025	—2.4	+.0050	—1.7	—.0144	—1.5	—.0021	—0.1
May,	120	29.4034	63.1	+.0108	+0.3	—.0002	+2.2	—.0297	—0.3	—.0082	—0.6
June,	132	29.5063	63.7	+.0036	+1.5	—.0084	+2.8	—.0161	+1.8	—.0135	+1.6
July,	126	29.2452	63.2	+.0056	+1.2	—.0093	+1.8	—.0186	+1.5	+.0021	+0.5
Aug.	131	29.3716	63.4	+.0209	+1.2	+.0139	+1.8	—.0099	+1.6	—.0052	—0.1
Sept.	86	29.4750	61.6	+.0121	+0.5	+.0291	+2.5	—.0101	+1.0	+.0185	+0.2
Oct.	128	29.5492	58.8	—.0086	0.0	—.0137	+2.5	—.0330	+0.2	—.0006	+0.2
Nov.	144	29.3286	63.3	+.0180	—6.1	+.0245	—1.5	+.0052	—0.7	+.0038	+0.2
Dec.	117	29.2393	61.9	—.0257	—5.9	—.0004	—3.3	+.0218	—2.3	+.0037	+0.7
Sum,	1572	352.3773	727.7	+.0354	—17.9	+.0209	+2.3	—.1409	—7.2	+.0025	+2.0
Mean,		29.3648	60.6	+.0029	— 1.5	+.0017	+0.2	—.0117	—0.6	+.0002	+0.2

TABLE III.—1829.

Month.	Number of Obs.	10 P. M.		8 A. M.		10 A. M.		4 P. M.		8 P. M.	
		Barometer.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.
		Inches.	°	Inches.	°	Inches.	°	Inches.	°	Inches.	°
Jan.	139	29.5066	58.2	—.0058	—6.3	+.0011	—2.7	—.0105	+0.2	—.0032	+1.1
Feb.	81	29.7275	62.2	+.0324	—5.9	+.0473	—1.7	+.0035	—1.1	+.0041	+0.4
Mar.	126	29.4268	60.7	—.0055	—4.3	+.0001	—0.1	—.0190	—1.3	—.0040	—0.7
April,	138	29.1368	60.8	—.0356	—3.9	—.0334	+0.8	—.0287	0.0	—.0092	—1.0
May,	147	29.6175	61.4	—.0097	+0.3	—.0077	+0.7	—.0227	—0.5	—.0047	—1.0
June,	51	29.4425	65.5	+.0068	—0.4	—.0087	+0.4	—.0266	+0.2	—.0100	—0.3
July,	107	29.2338	64.7	+.0078	+0.8	—.0022	+0.8	—.0096	+0.3	—.0072	—0.2
Aug.	79	29.4656	64.5	+.0064	+0.6	+.0099	+0.9	—.0099	—0.1	—.0042	—0.3
Oct.	117	29.4712	60.8	+.0202	—2.8	—.0065	—0.3	+.0123	—0.6	+.0104	0.0
Nov.	122	29.6093	61.4	+.0079	—4.1	+.0308	—2.0	—.0026	—1.6	—.0013	0.0
Dec.	115	29.7644	56.4	—.0273	—5.0	—.0348	—3.6	—.0169	—2.0	—.0031	—0.1
Sum,	1222	324.4020	676.6	—.0024	—31.0	—.0041	—6.8	—.1307	—6.5	—.0324	—2.1
Mean,		29.4911	61.5	—.0002	— 2.8	—.0004	—0.6	—.0119	—0.6	—.0029	—0.2

TABLE IV.—1830.

Month.	Number of Obs.	10 P. M.		8 A. M.		10 A. M.		4 P. M.		8 P. M.	
		Barometer.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.	Barom.	Attach. Ther.
		Inches.	°	Inches.	°	Inches.	°	Inches.	°	Inches.	°
Jan.	121	29.7290	55.1	—.0063	—3.9	—.0437	—2.1	—.0090	—0.5	—.0069	+0.4
Feb.	110	29.3549	57.8	—.0069	—5.2	+.0308	—2.2	+.0071	—1.1	+.0027	—0.6
Mar.	116	29.5025	60.6	+.0119	—2.5	—.0270	—0.7	—.0137	—0.9	—.0007	0.0
April,	86	29.2313	61.0	+.0065	—1.6	—.0090	+0.7	—.0218	—0.9	+.0041	—0.3
May,	131	29.4119	60.4	+.0235	+1.1	+.0222	+1.2	—.0140	+0.2	+.0039	—0.6
June,	126	29.4197	62.3	—.0161	+0.5	—.0292	+0.8	—.0321	+0.1	—.0058	—0.2
July,	140	29.4428	65.8	—.0135	+1.0	—.0158	+1.0	—.0144	+0.2	—.0117	—0.1
Aug.	85	29.3519	64.1	+.0052	+1.3	+.0086	+0.3	+.0009	—0.5	+.0065	—0.7
Sept. } Oct. }	54	29.2583	62.7	+.0323	—2.1	+.0230	—0.2	+.0243	—2.3	+.0193	—1.2
Nov.	123	29.1858	59.2	—.0130	—2.0	+.0129	—0.4	—.0060	—0.8	—.0061	+0.2
Dec.	62	29.3108	58.3	+.0132	—2.9	+.0403	—2.0	+.0119	—0.5	+.0076	+0.2
Sum,	1154	323.1989	667.3	+.0368	—16.3	+.0131	—3.6	—.0668	—7.0	+.0129	—2.9
Mean,		29.3817	60.7	+.0033	— 1.5	+.0012	—0.3	—.0061	—0.6	+.0012	—0.3

TABLE V.

SPRING PERIOD.

Month and Year.	Number of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
		Inches.	Inches.	Inches.	Inches.	Inches.
March 1828,....	152	29.3571	29.3511	29.3295	29.3477	29.3473
April 1828,.......	143	29.1964	29.2039	29.1845	29.1968	29.1989
May 1828, ...	120	29.4142	29.4032	29.3737	29.3952	29.4034
March 1829,....	126	29.4213	29.4269	29.4078	29.4228	29.4268
April 1829,.......	138	29.1012	29.1034	29.1081	29.1276	29.1368
May 1829, ...	147	29.6078	29.6098	29.5948	29.6128	29.6175
March 1830,....	116	29.5144	29.4755	29.4888	29.5018	29.5025
April 1830,.......	86	29.2378	29.2223	29.2095	29.2354	29.2313
May 1830, ...	131	29.4354	29.4341	29.3979	29.4158	29.4119
Sum, ...	1159	264.2856	264.2302	264.0946	264.2559	264.2764
Mean, ...		29.3651	29.3589	29.3438	29.3618	29.3640

TABLE VI.

SUMMER PERIOD.

Month and Year.	Number of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
		Inches.	Inches.	Inches.	Inches.	Inches.
June 1828,.......	132	29.5099	29.4979	29.4902	29.4928	29.5063
July 1828, ...	126	29.2508	29.2359	29.2266	29.2473	29.2452
August 1828,...	131	29.3925	29.3855	29.3617	29.3664	29.3716
June 1829,.......	51	29.4493	29.4338	29.4159	29.4325	29.4425
July 1829, ...	107	29.2416	29.2316	29.2242	29.2266	29.2338
August 1829,...	79	29.4720	29.4755	29.4557	29.4614	29.4656
June 1830,.......	126	29.4036	29.3905	29.3876	29.4139	29.4197
July 1830, ...	140	29.4293	29.4270	29.4284	29.4311	29.4428
August 1830,...	85	29.6571	29.3605	29.3528	29.3584	29.3519
Sum, ...	977	264.5061	264.4382	264.3431	264.4304	264.4794
Mean, ...		29.3896	29.3820	29.3715	29.3812	29.3866

TABLE VII.

AUTUMN PERIOD.

Month and Year.	Number of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
		Inches.	Inches.	Inches.	Inches.	Inches.
Sept. 1827,......	50	29.5425	29.5614	29.5437	29.5743	29.5767
Oct. 1827,......	138	29.3221	29.3199	29.3096	29.3265	29.3271
Nov. 1827,......	145	29.5669	29.5636	29.5650	29.5391	29.5311
Sept. 1828,... ..	86	29.4871	29.5041	29.4649	29.4935	29.4750
Oct. 1828,......	128	29.5406	29.5355	29.5162	29.5472	29.5492
Nov. 1828,......	144	29.3466	29.3531	29.3358	29.3324	29.3286
Oct. 1829,......	117	29.4914	29.4647	29.4835	29.4816	29.4712
Nov. 1829,......	122	29.6172	29.6401	29.6067	29.6080	29.6093
Sept. Oct. } 1830, ...	54	29.2906	29.2813	29.2826	29.2776	29.2583
Nov. 1830,......	123	29.1728	29.1987	29.1798	29.1797	29.1858
Sum, ...	1107	294.3778	294.4224	294.2858	294.3599	294.3123
Mean, ...		29.4378	29.4422	29.4286	29.4360	29.4312

TABLE VIII.

WINTER PERIOD.

Month and Year.	Number of Obs.	8 A. M.	10 A. M.	4 P. M.	8 P. M.	10 P. M.
		Inches.	Inches.	Inches.	Inches.	Inches.
Dec. 1827, ...	129	29.0764	29.0853	29.0813	29.0833	29.0804
Jan. 1828, ...	151	29.3977	29.4076	29.4135	29.4237	29.4211
Feb. 1828, ...	142	29.3062	29.2815	29.2807	29.2922	29.2914
Dec. 1828, ...	117	29.2136	29.2389	29.2611	29.2430	29.2393
Jan. 1829, ...	139	29.5008	29.5077	29.4961	29.5034	29.5066
Feb. 1829, ...	81	29.7599	29.7748	29.7310	29.7316	29.7275
Dec. 1829, ...	115	29.7371	29.7296	29.7475	29.7613	29.7644
Jan. 1830, ...	121	29.7227	29.6853	29.7200	29.7321	29.7290
Feb. 1830, ...	110	29.3480	29.3857	29.3620	29.3576	29.3549
Dec. 1830, ...	62	29.3240	29.3511	29.3227	29.3184	29.3108
Sum, ...	1167	294.3864	294.4475	294.4159	294.4466	294.4254
Mean, ...		29.4386	29.4447	29.4416	29.4447	29.4425

EXPLANATION OF PLATE VII.

This Diagram is intended to exhibit the curve by which the Horary Oscillation of the Barometer is connected with the Latitude.

The axis AX represents the quadrant of latitude extended into a straight line; and the distances of the points which are laid down on either side of it, give the projections upon an arbitrary scale of the amounts of barometric oscillation. The ordinates are reckoned *plus* to the right hand of the axis, and *minus* to the left. The curve YM*y* drawn through these points, gives the best expression of the law which connects them, the sum of the squares of the deviations of the observed ordinates from the computed ones being reduced to a minimum, as was shewn in the preceding paper, this curve being merely a mechanical projection of the final equation.

The amount of the equatorial ordinate AY is 2.650 millimetres. That of the polar ordinate X*y* is —0.381 millimetres. The latitude at which the oscillation changes its sign, or where the curve cuts the axis at M, is 64° 8′ 6″.

Fig. 1

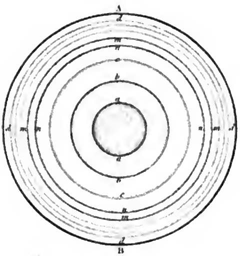


Fig. 2

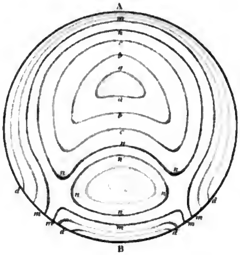


Fig. 3

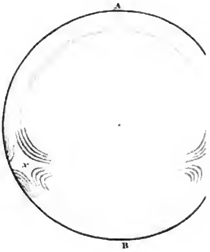


Fig. 4

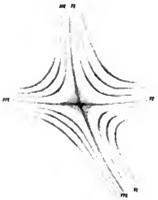


Fig. 5

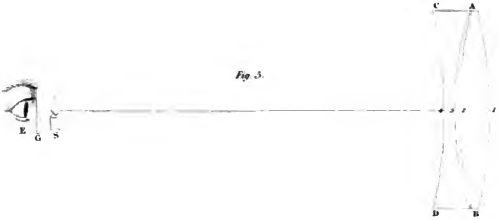
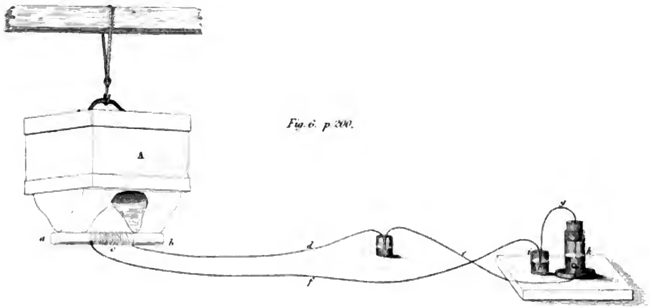


Fig. 6. p 200.



On a new Species of Coloured Fringes, produced by reflexion between the Lenses of Achromatic or Compound Object-Glasses. By DAVID BREWSTER, LL.D., F. R. S., V. P. R. S. Ed. &c.

(Read January 2. 1832.)

IN a paper which I communicated to this Society in 1815, and which was published in the Seventh volume of their Transactions, I described a new species of coloured fringes, produced between two plates of parallel glass. From a consideration of the theory of this class of phenomena, it was obvious that analogous, though much more complicated, systems of rings should be produced between plates with curved surfaces, but it was not till 1822 that I succeeded in detecting them; and so completely are these rings concealed by the superposition of similarly situated images, that, in consequence of having forgotten my method of observation, I have experienced the greatest difficulty in rediscovering them.

My earliest experiments were performed with a double achromatic object-glass, made by BERGE, having a diameter of $2\frac{3}{10}$ inches, and 30 inches in focal length. The inner surfaces of the crown and flint glass lenses were ground to different radii, as shewn in the section of it at AB, CD, Fig. 5. Plate IX.; and the outer surface of the flint-glass lens was concave, so that there was left between the lenses a meniscus of air A 2 B 3 A.

In order to observe the system of rings as nearly as possible at a perpendicular incidence, I placed the smallest flame I could procure at S, about four or five inches distant from the object-

glass AD, and interposing a small screen G between the flame and the eye at E, I held the eye as close to S as possible, and varied the distance of the object-glass till the inverted greenish-coloured flame * reflected interiorly from the concave surface A 1 B seemed to cover the whole area of the object-glass. When this is accomplished, the rings may, by a slight change in the position of the object-glass, or by screening the image formed by one reflection from A 1 B, be distinctly seen over the expanded but enfeebled image formed by a second reflection from the same surface.

When the flame is very small, and the eye sees it projected against the centre of the object-glass, the rings are grouped into a concentric system, as shewn in Fig. 1, approaching closer and closer to each other as they advance from the centre to the circumference of the lens. Two of these rings, *mmmm*, *nnnn*, having an intermediate position in the system, are distinguished from the rest by their darkness, and by the whiteness of the light between them; and they enjoy the remarkable property of becoming the bounding lines of *four* systems of fringes, into which the general system is subdivided by oblique reflection.

In order to observe this interesting change, incline the object-glass so that the point A is farther from the eye than B, and so that the eye receives the rays that are reflected obliquely from every point of the surface A 1 B. At a very slight deviation from a perpendicular incidence, the rings will become smaller and closer on the side A, and broader and wider on the side B, having intermediate breadths and distances at intermediate points of the circumference between A and B. By increasing the incidence, the inner ring *aa*, Fig. 1, contracts into a sort of irregular crescent *aa*, Fig. 2. The second and third rings, *bb*, *cc*, Fig. 2.

* This flame has a greenish-colour, in consequence of the rays which form it having passed through twice the thickness of the crown-glass lens A B.

do the same as shewn at *bb*, *cc*, Fig. 2., and at a greater incidence, the dark ring *nn*, Fig. 1, assumes a similar form *nnnn*, Fig. 2., and forms the boundary of the *remote central system ncbaabcn*. In like manner, the lower part of the ring *nn*, Fig. 1, has enclosed a smaller but similar system of rings, which are shewn at *n′ n′ n′ n′*, and may be called the *near central system*. While these changes are going on, the rings without *nn*, Fig. 1, are undergoing analogous, though opposite, inflexions. The outermost *ddd*, Fig. 1, divides itself into two unequal portions, which run out into the circumference at the points *d*, *d*, *d′*, *d′*, Fig. 2. Then the next ring, viz. the dark one, *m*, *m*, *m*, *m*, forming the boundary of the *remote external system m*, *m*, *m*, A, and of the *near central system m′ m′ m′* B.

The four groups of rings thus developed, assume, at greater incidences, the character shown in Fig. 3, but they are not seen all at once; and in tracing their form, it is necessary to cause the image on which they are produced, to be reflected successively from different parts of the lens. The rings are so closely packed together at a distance from the centres *x*, *x*, to which they are all related, that it is extremely difficult to perceive them. By increasing the incidence still farther, the rings close in upon the centres *x*, *x*, and become exceedingly close and numerous. The points *x*, *x* approach to the circumference of the lens, and the rings become more luminous from the increase in the reflected light, at increased obliquities of incidence.

In some object-glasses the rings are exceedingly numerous and close, whether seen as in Fig. 1, or as in Fig. 3; and when this is the case, the black rings *m*, *n*, and the centres *x*, *x*, are near the circumference. In other object-glasses, particularly in a large one of Tulley's, in the Calton Hill Observatory, the rings are very few in number, and the dark fringes *m*, *n*, and the centres *x*, *x*, are advanced considerably from the circumference towards the centre of the lens. In this case the rings

are more easily seen, and they undergo very beautiful modifications in passing from a perpendicular to an oblique incidence.

There can be little doubt that this variation in the size and number of the rings depends on the thickness of the meniscus of air between the lenses; but in order to put this to the test of experiment, I separated the two lenses A B, C D, Fig. 5, and I found the rings to increase in number, and diminish in breadth, in proportion to the distance of the two lenses. Hence it follows, that, in all those object-glasses where the inner surfaces are coincident, or are cemented by mastic or other varnishes, no rings will be produced,—and that the number of the rings furnish us with a measure of the difference of curvature of the inner surfaces of the combined lenses.

In some of the oblique systems of rings which I have observed, the outer fringe *n* of one of the central systems approached so near the outer fringe *m* of one of the external systems, that the space between them was *straw-yellow*, in place of white; and in one case, the four bounding fringes united, and formed a black cross, as shown in Fig. 4.

In a large double object-glass, made by Gilbert, 3.8 inches in diameter, and in a similar one by Dollond, 2.75 inches in diameter, the rings could only be seen by looking through the convex side A 1 B, Fig. 5. In the first of these lenses there were only two fringes in the near central system of rings, so that the inner surfaces must have been nearly coincident.

If we separate the lenses a little at A, Fig. 1, and Fig. 5, the system of rings approaches the edge B, and become more numerous and more close to each other. The other systems close, and become concentric to them, and the whole become an elliptical system.

When the lenses are separated a little at B, Fig. 1, and Fig. 5, the system enlarges, and the rings grow more numerous, the other systems becoming concentric with them, and forming a close system.

In a triple object-glass, which gave a system of rings similar to that in Fig. 3, I observed them to be crossed with another system of minute fringes parallel to one another, and to the line joining the centres *x* and *x*. The object-glass which exhibited this curious effect is not now within my reach, so that I am unable to give any farther account of this new system.

In order to determine the surfaces of the double object-glass, AD, Fig. 5, which are essential to the production of the rings, I covered the convex surface A1B with oil of nearly the same refractive power as glass, and the rings wholly disappeared. Having removed the oil, I filled with the same fluid the space or hollow meniscus between the lenses, when the rings again disappeared. The lenses being again cleaned, I removed CD, and could no longer observe any fringes. Hence, it follows, that the action of the two surfaces of the convex lens, and the inner surface of the concave one, are necessary to the production of the fringes.

From these facts, it will appear, that the coloured rings arise from the interference of two pencils of light, one of which has suffered *three* reflexions within the convex lens AB, and has passed *four* times through its thickness, with another pencil which has suffered *two* reflexions within the convex lens, and *one* reflexion from the inner surface of the concave lens, and has passed four times through the thickness of the convex lens, and twice through the thickness of the meniscus of air.

When the light is incident perpendicularly on the centre of the lens, the interval of retardation, or the difference between the lengths of the paths of the two rays, is equal to twice the greatest thickness of the meniscus of air. Hence, if this thickness is very small, the tints corresponding to it will be distinctly observed; but if the thickness is considerable, as it often is, the tints will belong to such high orders, that they will only be seen when a small flame of homogeneous light is used.

As the incident ray advances from the centre towards the circumference, the meniscus of air diminishes in thickness, and al-

so the interval of retardation, so that the orders of the rings descend, as in Fig. 1. But there is a particular point between the rings *m* and *n*, where the interval of retardation is nothing, or where the lengths of the paths of the two interfering pencils are equal, so that we have a *white* ring at that place. Beyond this, the interval of retardation becomes perceptible, and another system of rings commences, rising to their highest order at the very circumference of the object-glass.

When the eye and the flame are in the axis of the object-glass, the isochromatic lines are circles; but at oblique incidences they have the singular forms shewn in Figs. 2 and 3, the line where there is no interval of retardation being the boundary of the four different systems of fringes shewn in these figures.

As the paths of the interfering pencils are performed in three media, crown-glass, flint-glass, and air, and as their lengths vary very quickly and irregularly, as the angle of incidence varies, and as the point of incidence changes its position, the analytical expression of the interval of retardation will be very complex.

Account of some Experiments in which an Electric Spark was elicited from a Natural Magnet. By James D. Forbes, Esq. F.R.S.E., F.G.S., &c.

(*Read* 16*th April* 1832.)

Notwithstanding the intimate connexion which has long been known to exist between magnetism and electricity, we may safely say, that, only fifteen years ago, the announcement of the excitation of a luminous spark from a natural magnet would have been received with astonishment and even with incredulity.

After the great discovery of electro-magnetism, in 1819, by Professor Oersted, the extreme improbability of such a discovery was indeed removed; but, even after that period, until the recent researches of our distinguished countryman Mr Faraday, every attempt having failed to procure the feeblest trace of electricity from the obdurate magnetic mass, so striking a result, which comes home to the comprehension of those least accustomed to interest themselves in the less palpable results of scientific enquiry, could scarcely have been looked for with any degree of confidence.

The beautiful experiments of Mr Faraday, the fundamental facts of which I had the honour to lay before the Society at their last meeting, pointed out the path for arriving at this fine result, and enabled us to appreciate the probability of attaining it. Having had the good fortune conclusively to establish so

interesting an experimental truth, though not, if report tells true, until after Signor Nobili, the ingenious and persevering philosopher of Reggio, I have taken advantage of this last meeting of the Society for the season, to bring the subject before them: and though I regret that, from the nature of the experiment, it can only be shewn to one or two persons at once, I believe that there are several individuals now present, who can bear ocular testimony to the production of the spark.

The discovery of Mr Faraday has conclusively demonstrated, that in every case where a magnetic current is created (to use the word *current* in its ordinary acceptation, as indicative of a peculiar condition, and without reference to any theory whatever), a momentary electric current is induced at right angles to it. The experiment may be shewn in two ways: either by mechanically causing a magnetic bar to traverse the axis of a helix of copper-wire of considerable length,—or by causing a piece of soft iron, placed in the axis of such a helix, to connect the poles of a horse-shoe magnet, and thus temporarily acquire polarity. In both cases, the current of electricity is most easily shewn by its action on the common Multiplier, the extremities of the wires of which are connected with those of the copper-wire forming the helix, and which, from its rectangular position at every point to the magnetic current caused to traverse its axis, collects the electricity developed in that direction. By both methods, the current is only instantaneous; and in the first case, if the magnet be caused to make a reciprocating motion in the axis of the helix, the currents are of course excited in opposite directions, and, by accommodating these to the alternate vibrations of the needle of the Multiplier, it may, by means of a small bar, be made to oscillate in large arcs.

The second method, however, where the magnetic current is created through a piece of soft iron, by causing it to connect the poles of a large magnet, is that which in my late experiments

I have entirely employed; and the subject of them has been a very fine natural magnet, capable of supporting 170 lb. presented to the University by Dr Hope. I willingly avail myself of this opportunity to express my obligations to that gentleman for the numerous and important facilities which have been afforded to my researches, in his laboratory, where the magnet still is.

My preliminary experiments demonstrated, by the action upon the multiplier and upon the frog, that a very powerful and instantaneous current of electricity was conveyed through the helix at the moment of making the contact of the connecting iron with the magnet. My attention was then directed to such an arrangement of apparatus as should by means of this current produce a spark. Mr Faraday had actually accomplished this in the case of an electro-magnet, that is, by the conversion of a soft iron bar into a magnet, by a galvanic current revolving round it. He had, from another portion of the bar, been able to reproduce the electricity, and obtain a spark from it.

In an early stage of my experiments I had, as far back as the 30th of March, obtained a spark from the magnet, which, however, being unable to repeat, from circumstances of which I afterwards became aware, I did not choose to publish at the time. I accordingly proceeded closely to investigate the circumstances under which sparks were to be obtained from feeble galvanic currents of low intensity. I used the common cylindrical electro-magnetic battery, in which, by varying the charge of acid, I could obtain any required power. Thus I adjusted it till I obtained from a momentary current nearly the same action on the Multiplier as I had developed by the magnet. Removing it into a dark place, I found that sparks were obtained at the instant of making and breaking the circuit connecting the cups of the battery. Satisfied that I had a sufficient current of electricity, I proceeded to apply to the magnet the conditions which I had found

most effectual for eliciting the spark. These were, 1*st*, That the spark is more easily obtained at the instant of interrupting than that of completing the galvanic circuit: 2*d*, That of the combinations which I tried, a fine pointed iron-wire suddenly withdrawn from contact with a surface of pure mercury, forming part of the circuit, was the most regular in exciting the spark, and that a good deal depended upon the suddenness of the interruption; and, 3*d*, That the spark was easiest obtained from the mercury, not at the horizontal upper surface, but where capillary action attracted it to the sides of the containing vessel; and that this was independent of the material of the vessel, being the same with wood, glass, and metal.

These precautions are all easily resolved into the general one—that the circuit conveying a weak current should be as abruptly divided as possible, in order to the production of a spark; and from the greater power of slender wires formed of imperfect conductors, such as iron, to accumulate the effects of *weak* currents *, that part of the apparatus is also explained. The action of the adhesion of mercury to the vessel is probably to render its connection with the wire inserted at that point more perfect.

It will be unnecessary at present to recount the difficulties which still obstructed my progress, though some of them were, I think, of a kind not unimportant to electro-magnetic science. I shall, however, only briefly notice the arrangement of the apparatus with which, on the 13th of April, I succeeded in obtaining the spark at pleasure.

The large natural magnet is represented at A, Fig. 6, Plate IX. A cylindrical connecter of soft iron, *a b*, passing through the axis of the helix *c*, was made to connect the poles

* See Mr Harris's Paper in this volume.

of the magnet; accuracy of contact was found to be of considerable importance to the success of the experiment, and one side of the cylinder was carefully formed to a curve of about two inches radius for this purpose. I found great advantage from a mechanical guide, not represented in the figure, to enable an assistant to bring up the connector rapidly and accurately to the magnet in the dark. The helix *c* consisted of about 150 feet of copper-wire, nearly one-twentieth of an inch in diameter, $7\frac{1}{2}$ inches long, and containing four layers in thickness, which were carefully separated by insulating partitions of cloth and sealing-wax. The one termination *d e* of the wire, passed into the bottom of a glass tube *h*, half filled with mercury, in which the wire terminated, and the purity of the mercurial surface is of great consequence to the experiment. The other extremity *f* of the helical wire communicated by means of the cup of mercury *i*, with the iron-wire *g*, the fine point of which may be brought by the hand into contact with the surface of the mercury in *h*, and separated from it at the instant when the contact of the connector *a b* with the poles of the magnet is effected. The spark is produced in the tube *h*.

The success of the experiment clearly depends on the synchronism of the production of the momentary current by connecting the magnetic poles, and the interruption of the galvanic circuit at the surface of the mercury. This might be pretty nearly ensured by a variety of simple mechanical contrivances which suggest themselves,—but as these would require very considerable nicety in their execution, I have been satisfied with the precision which may be insured by a good ear and an accurate assistant,—as I have thus, with a little practice, been able to produce, for many times in succession, at least two sparks from every three successive contacts.

These sparks have generally a fine green colour; that I obtained on the 30th of March was in every respect similar to those

I afterwards procured. The intensity of light varies considerably, as it depends on the degree of accuracy with which the circuit is broken at the moment of contact. Sometimes it is highly vivid, and has been seen some yards off in a dark place.

As soon as I had the circumstances under my command, I hastened to show the experiment to my brother, who was present, and to Dr Gregory, acting secretary of this Society. I afterwards had the satisfaction of showing it to Dr Hope, to Sir John Leslie, and several other gentlemen.

I have now stated, I hope not with fatiguing minuteness, the mode by which I have arrived at a result of some interest for science—of that striking character, too, which at once seizes the imagination and the attention, and which may even give it a degree of importance superior to what, weighed in the balance of calm philosophy, it may perhaps deserve. The multiplier and the frog are to the eye of science as sure tests of an electric current, as is the spark to the eye of sense.

I beg to repeat, that the success of Signor Nobili's experiment is only known to me through the medium of the public prints; I am quite ignorant of the channel by which the report reached this country; and, at all events, not the slightest clew has been given as to his mode of arriving at the result.

Let the minor circumstances turn out as they may, all who have had any share in this interesting research, must agree in giving to Mr Faraday the great, almost the sole, merit of a discovery, of which his researches formed the basis, and whose liberality in throwing open to the scientific world an interesting truth, which he might fairly have retained until he had worked it out in its various details, merits as much praise as the originality and fertility of his genius.

POSTSCRIPT.

SINCE the preceding paper was read, and placed in the hands of the printer, I have seen the Account of the experiments of Signori NOBILI and ANTINORI, contained in the number of the *Annales de Chimie et de Physique*, dated December 1831; and I have likewise, by the kindness of Mr FARADAY, received a copy of his paper about to be published in the Philosophical Transactions. From these documents, it is established, 1*st*, That Mr FARADAY obtained a spark from a temporary or electro-magnet, as far back as November 1831. This I stated to have been the case in the preceding paper, upon Mr FARADAY'S authority, who informed me of it about two months ago; and this was the "*cas particulier*," mentioned in the French version of Mr FARADAY'S letter to M. HATCHETTE, read to the Academy of Sciences, which gave rise to the experiments of Signori NOBILI and ANTINORI, and who also allude to it in their paper, without knowing the real circumstances of the experiment *. It appears, 2*dly*, That the first document giving an account of the excitation of a spark by these philosophers, from a *permanent* or *natural* magnet, is dated from the Museum at Florence 31st January 1832, was published in the Antologia, bearing the date of November 1831, and afterwards translated into the *Annales de Chimie*, bearing the date of December. "It is evident," says Mr FARADAY, speaking of the former, "the work could not have been then printed, and though Signor NOBILI in his paper has inserted my letter as the text of his experiments, yet the circumstance of the back date has caused many here, who heard of NOBILI'S experiments by report only, to imagine his results were anterior to, instead of being dependent upon mine †.

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* Annales de Chimie, Dec. 1831, pp. 403, 417.

† Phil. Transactions for 1832, p. 162, note.

The notice of Signor Nobili's experiment, to which I have alluded in my paper as having reached me whilst my investigations were in progress, was that contained in the Literary Gazette for March 24, stating simply the report of the fact, though without naming any authority. I learn from Mr Faraday, that it appeared there by a circuitous channel of information, actually derived from Signor Nobili's communication to himself. The first information I had of Nobili's method of making the experiment, which was in its simplest form almost the same with my own, and explained in terms nearly identical, was not till the *Annales de Chimie* for December reached my hands, which was on the 30th of April, when the foregoing paper was in the press.

I take this opportunity of adding, that the experiment upon the frog, simply mentioned above, was made at the suggestion and with the assistance of Dr Hope, on the 3d of April. It appears from Mr Faraday's paper*, that he was not successful in exciting muscular action by Dr Knight's great compound magnet belonging to the Royal Society, but he afterwards obtained the effect by making a very sudden rupture of contact of the armature of a smaller magnet. The same result was obtained by Nobili and Antinori†, who justly remark the striking proof which it affords of the extreme delicacy of the Frog Galvanometer, since the magneto-electric currents (to use the language of Faraday) resemble the thermo-electric ones in the very great difficulty with which they pass through moist conductors.

Finally, as far as yet known, no one except Signori Nobili and Antinori and myself have yet obtained the spark from the natural or permanent magnet. This, indeed, must be in a great measure owing to the power of the magnets we have been

* Article 56.

† Annales de Chimie, Dec. 1831, p. 425.

able to command, (no notice is given of the size of that at Florence); there is little doubt, however, from the constancy and brilliancy of the results I have obtained, that, by following the same method, the experiment may be repeated with smaller apparatus. At least it is in the highest degree probable that it will be obtained with Dr GOWIN KNIGHT's immense artificial battery, consisting of 450 bars, 15 inches long, which was employed by Mr FARADAY and Mr CHRISTIE in the prosecution of the Experiments on Magneto-Electric Induction.

GREENHILL, EDINBURGH,
7th May 1832.

On a New Electrometer, and the Heat excited in Metallic Bodies by Voltaic Electricity. By WILLIAM SNOW HARRIS, Esq. F.R.S.

(Read December 19. 1831.)

1. IN the course of some inquiries concerning the power of metallic substances to conduct intense electrical explosions, an account of which was honoured by a place in the Philosophical Transactions for the year 1827, I sought to obtain a comparative measure of the conducting power, in the heat evolved by the metal, at the time of transmitting the charge. The instrument employed for this purpose, was, in principle, that of a common air thermometer, the given metal, the subject of experiment, being drawn into a wire, and passed air-tight across its bulb. This simple contrivance, I have since extended to the general purposes of an electrometer, so as to estimate the force of any ordinary electrical accumulation. The results arrived at with voltaic combinations, seem, for the most part, of much practical utility; and therefore some account of them may, I trust, be found worthy of the attention of the Royal Society of Edinburgh.

2. The electrometer, as prepared for the purposes of voltaic electricity, will be easily understood by the following description:—*abc*, Fig. 1, Plate VIII., is a fine glass-tube, whose interior diameter is about the $\frac{1}{10}$th of an inch; there is a spherical reservoir blown at *a* for the reception of a coloured fluid; from this

Fig. 1.

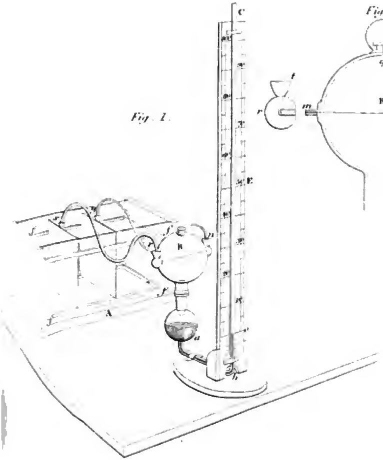


Fig. 2

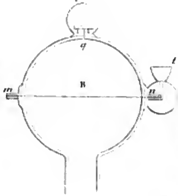


Fig. 3.

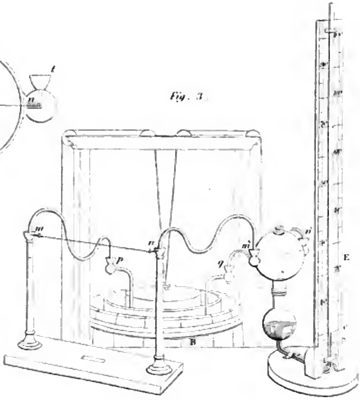


Fig. 4.

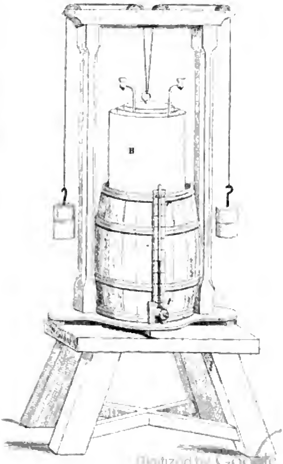
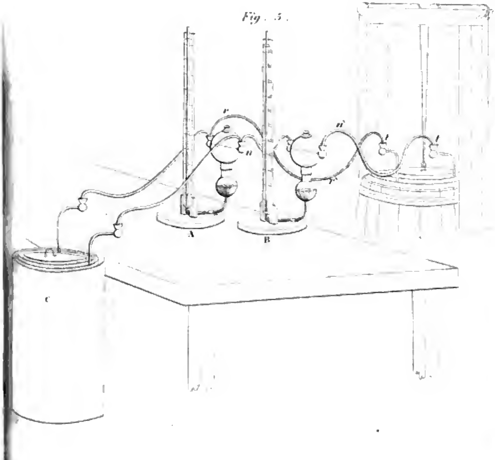


Fig. 5.



the tube is bent a little forward, and then perpendicularly, and is finally secured to a correctly divided scale *bc*, sustained on a convenient base, as seen in the figure. The point *o* on the scale, corresponding to the level of the coloured fluid in the opposite reservoir *a*, is marked zero.

A metallic wire *mn*, Fig. 2, varying from the $\frac{1}{100}$th to the $\frac{1}{3}$th of an inch in diameter, according to the circumstances of the experiment, is passed air-tight across the glass ball B by means of small flanges of brass *mn*, cemented in and round two holes drilled through the sides, and upon these are screwed two brass balls *rn* in such way as to render the whole air-tight. The method of fixing the wire is extremely simple, the brass parts being made quite clean internally; the wire is passed directly through them; it is put gently on the stretch, and is then compressed in the holes by small pegs of tough wood, so as to insure a good contact: the pegs, and extremities of the wire, are allowed to project a little, for the convenience of removal, and thus different wires may be substituted with great expedition. The ball B, being thus prepared, is screwed air-tight on the spherical reservoir *a*, Fig. 1, by the ordinary intervention of brass caps, which should be closely cemented with good sealing-wax to the glass; hence, when the wire *mn* becomes heated by the voltaic action, the fluid will be observed to ascend along the scale *bc*.

The contact with the battery, as in Figs. 1, 2, 3, &c., is made by means of the small brass cups *tt*, Fig. 2, screwed in the balls *rn*, in which is placed a little mercury; and, to adjust the fluid to zero of the scale, there is a small opening drilled through the upper part of the bulb at *q*, Fig. 2, which, being covered also with a ball and flange, admits of a communication being easily made with the external air at pleasure *.

* The fluid for marking the degrees of action may consist of rectified spirit one part, distilled water three parts, coloured with tincture of cochineal; to which may

3. The indications of this instrument appearing, after a few trials, to be very general and decisive, I endeavoured to ascertain how far it might be relied on as a measure of electrical action, as also the variations to which it might be subject: as these points became satisfactorily determined in the course of the following investigation, I shall give a brief detail of the experiments as they occurred.

4. (*a*) My first attempts were directed to the law of the action of a single zinc and copper plate *xy*, Fig. 1, placed at various distances from each other in a dilute acid. The plates employed for this purpose were each 7 inches high, 6 inches wide, and about the $\frac{1}{10}$th of an inch thick, and were secured in a light frame of varnished wood *ff*. The cell A for the acid had bottom and ends of varnished mahogany, and sides of glass; it was internally 10 inches long, 8 inches deep, and 7 inches wide. The wire in the electrometer B was of copper, and about the $\frac{1}{100}$th of an inch in diameter *. The dilute acid consisted of nitrous acid one part, sulphuric acid one part, water forty-eight parts by measure. The contacts with the plates were effected through small cups *xy* containing mercury; the wires employed to complete the circuit being of copper, and $\frac{1}{4}$th of an inch in diameter.

The following are the results of this experiment:—

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be added as much sulphuric acid as will render the whole pleasantly sour. Mercury also, when the tube is fine, may be used with advantage.

* This experiment was likewise contemplated by my friend Mr Watkins, the curator of the apparatus, in the London University. In the course of a few trials with the instrument which I had the pleasure of making with him at his house, we succeeded in rendering sensible, the action of a small plate of zinc and copper, of only $\frac{1}{4}$th of an inch square, when placed in an extremely diluted acid, at upwards of 4 inches apart; the wire in the electrometer was in this instance *extremely* fine.

TABLE I.

Distance of Plates in inches.	Effect on Electrometer in degrees.
8.0	1.0
6.0	1.5 —
4.0	2.0
2.0	4.0
1.0	8.0
0.5	15.0 —
0.25	25.0

The law in this case seems, therefore, to be that of a simple inverse ratio of the distance between the plates, except in the near approximations, where the numbers are somewhat less than might have been anticipated. In repeating this experiment, however, the effect on the instrument was, for the most part, exactly in the inverse ratio of the distance between the plates.

The plates were removed from the acid before observing the effects due to each respective distance, and carefully wiped, and the acid occasionally changed, from a common stock previously mixed in mass.

5. (*b*) With a view of ascertaining the law of the action as regarded the quantity of metal immersed, the plates were divided, by horizontal lines drawn across them, into seven portions, each an inch wide, and as much dilute acid poured into the cell as was equivalent to immerse one, two, three, &c. of the divisions successively. In the following Table are given the results of this experiment, the plates being carefully wiped, and the acid renewed as before at such trial.

TABLE II.

Quantity of Metal immersed.	Effect in Degrees.
1	4
2	8
3	12 +
4	16
5	20
6	25
7	28

In this instance, therefore, the resulting effects are exactly as the quantity of metal immersed, or very nearly so, since the instrument appeared to vary with the increased power.

6. The foregoing experiments (5) being repeated with a very fine wire in the electrometer, of about the $\frac{1}{500}$th of an inch in diameter, the results were no longer regular, being as follows:—

TABLE III.

Quantity of Metal immersed.	Effect in Degrees.
1	6
2	10
3	13
4	15.5
5	18
6	19
7	20

7. The *comparatively* diminishing effects, corresponding to the increased action observable in the above Table, is fairly at-

tributable to some peculiar condition of the fine wire employed in the electrometer, by which it is rendered incapable of transmitting all the increased power; and it may be likewise observed, in comparing the above (Table III.) with the previous results (Table II.), that, when the action was comparatively feeble, then the fine wire (Table III.) was the most heated; but when the action became more considerable, then the larger wire (Table II.) was the most heated.

8. I repeated these experiments in various ways, and found the results very general and invariable. Thus, in employing three separate electro-magnetic batteries with double copper, each exposing about a square foot of zinc, the effects indicated by the electrometer, with extremely fine wires in the bulb, were nearly as great with one battery as with the three combined, whilst, on the contrary, the indications with larger wires were constantly proportionate to the increased power.

9. As the diminished effect on the electrometer, when a very fine wire is employed, with an increased charge, evidently depends on the imperfect conducting power of the wire, it seems reasonable to infer, that imperfect conducting fluids, such as water, or extensive quantities of very fine wire, may become almost insulators to this species of electrical accumulation; and such is found to be the case, since little or no effect is indicated by the instrument, with a given charge, when the circuit *xrny*, Fig. 1, consists of an extensive spiral of fine wire, covered with silk; and no effect *whatever*, when formed of water contained in glass tubes: hence the accumulation is either insulated, or otherwise transmitted so very imperfectly, as to be quite inappreciable by the heating effect.

10. The foregoing results are very analogous to those arrived at by Sir Humphrey Davy, in the course of his extensive and justly celebrated inquiries into the laws of voltaic action *: who

* Phil. Trans. for the year 1821, p. 435.

states, " That in a battery where the quantity of electricity is very great, and the intensity very low; charcoal, made to touch only in a few points, is almost as much an insulating body as water, and cannot be ignited, nor can wires of platinum be heated when their diameters is less than the $\frac{1}{30}$th of an inch, and their length 3 or 4 feet; and a foot of platinum-wire is scarcely heated by such a battery; whilst the same length of silver-wire is made red hot, and the same lengths of *thicker* wires of platinum are intensely heated." In this case, it is evident that the superior conducting power of the silver allows the quantity of electricity to pass through it, necessary to complete the heating effect; whilst the inferior conducting power of the platinum impedes the transmission of the charge; so that the heating effect is only evinced on thicker wires.

11. I endeavoured to find, by a variety of experiments, whether, on increasing the dimensions of the wires in the electrometer, any ratio greater than that of the increased power could be obtained, but I did not arrive at any new result. It is, however, not unlikely, that, provided the times of transmission were inversely proportional to the quantities of electricity transmitted; that is to say, if, in one-half the time, twice the quantity was discharged, and so on, then the indications of the electrometer might possibly be in a higher ratio, as I have invariably observed in the case of ordinary electrical explosions, by means of coated jars, in which the intensity of the action is such, that the whole accumulation is forcibly transmitted through the wire, in a dense form *.

12. Much care is requisite, in speculating on these curious phenomena, which appear to be intimately connected with the relations subsisting between the causes of heat and electricity; nevertheless the view taken by the celebrated philosopher above

* Memoirs of Plymouth Institution, p. 68. and 84.

mentioned *, namely, that the excitation of heat occasions the imperfection of the conducting power, seems, to a very great extent, extremely probable, as may be seen in the following experiments :

(*c.*) Having excited, somewhat powerfully, a large electro-magnetic battery (B. Figs. 3–4), of the cylindrical form, and containing about two square feet of zinc, with double copper ; the voltaic action was transmitted through the electrometer E, as in the previous experiments, by means of large copper-wires connecting the zinc and copper sides. In order to exemplify the influence of heat in decreasing the conducting power, about six inches of the circuit *mn*, Fig. 3, was made up of smaller wire, of about the $\frac{1}{80}$th of an inch in diameter, and to this wire, the flame of a spirit-lamp was applied, when the fluid in the electrometer E, had attained its greatest height on the scale ; the result was, that the fluid began to descend, as the wire *mn* became heated, and again recovered its former point of elevation on removing the lamp †.

(*d*) A somewhat similar result was obtained, in substituting for the wire *m n*, another wire of the same dimensions, but of worse conducting power ; in which case, the effect on the electrometer E was considerably diminished.

(*e*) When the wire *m n* was kept cool by evaporating a little ether from its surface, the effect on the electrometer was greatly increased.

(*f*) Similar results were obtained, when, instead of allowing the wire *mn* to form a part of the circuit, as in Fig. 3, it was made to connect the contact cups of the battery. In this case,

* Phil. Transactions for 1821, p. 438.

† This experiment with the electrometer had been previously tried by Mr Jonathan Hearder, of this place, to whose friendly assistance I have been occasionally much indebted, in the progress of these inquiries.

the electrometer E was only affected by as much of the charge as could not pass immediately in the direction of the small wire. When, therefore, the conducting power of the latter was caused to vary, either by change of temperature, size, or quality, such variation was simultaneously shewn by the elevation or depression of the fluid along the graduated scale of the electrometer, and of these changes the instrument seemed to be most delicately sensible.

13. (*g*) It does not appear to be of any consequence, how the heat is derived by which the conducting power becomes diminished; thus, when the wire *m n* was caused to transmit the charge of a second battery of sufficient power to heat it somewhat considerably, the same result ensued. This was very pleasingly shewn by the following arrangement: Two electrometers, A and B, Fig. 5, being placed in the circuit *t r′ r n′ t*, and the elevation of the fluid observed in each: one of them, A, was connected with a second battery C, and in such way, that the wire in the electrometer A might exclusively transmit the charge of this second battery, as well as that of the first *. In this instance, the elevation of the fluid in A was increased, whilst that in B was decreased; so that the two electrometers A and B appeared to vibrate as if delicately balanced, the slightest change in the one being accompanied by an opposite change in the other.

14. It may be inferred from the foregoing experiments, that the indications of the electrometer (2) may be always taken as a measure of the variations in the action of a voltaic combination, provided we employ a wire in the bulb above a certain diameter; and that the limit to the diameter of such a wire, in any given case, may be determined experimentally by observing the action

* This can be readily managed by one particular arrangement of the connecting wires, which admits of the two batteries operating separately.

of the battery, in three or four portions, both when separate and combined. Thus if the points of elevation of the fluid correspond with the respective quantities of metal in action, or very nearly so, supposing the combination arranged as a single pair of plates, we may fairly presume that the instrument will indicate any trifling variation of power upon the whole. It is requisite also to observe, that the steadiness of the fluid at a given point, was found to depend exclusively on the strength of the acid solution and state of the battery. When the zinc is moderately clean, and the action somewhat brisk, the power of the battery to maintain the fluid for a considerable time, at a constant altitude, is very decisive; on the contrary, when a weak acid is employed, or the zinc is loaded with oxide, then the effect is always more or less evanescent, being greatest at the instant of completing the circuit, after which the fluid sinks gradually along the scale, until it reaches some point at which the action of the battery may be considered constant; whilst, in other cases, no stationary point can be obtained, and the fluid sinks gradually to zero. When, however, the battery is in a good state for experiment, and the acid sufficiently strong, the fluid will be observed to rise steadily, and remain stationary at some given point of the scale. The circuit, however, should in all cases be made with very large copper wires, of at least one-fourth of an inch in diameter, and the contacts made good and perfect, in the usual way, by means of mercury.

15. The utility of the instrument, with some of the attendant circumstances, having been thus far investigated, I endeavoured to apply it, in a variety of instances, to the further elucidation of the phenomena of voltaic action, some few of which it may not be unimportant to notice.

(*h*) The zinc and copper plate, already described (4), Fig. 1, being exposed to the action of a dilute acid, and the resulting effect on the electrometer noted; a second and similar plate of

copper, previously placed on the other side of the zinc, was connected with the former, so as to double the quantity of copper. In this case, the effect on the electrometer, as might have been anticipated, was very considerably increased; it was, in almost every instance, at the time of making the contact, at least doubled. That a considerable increase of power is obtained by exposing both surfaces of the zinc to the influence of copper plates is already well known. It is in short the principle of the Wollaston arrangement of the voltaic series. But it is not perhaps so well known, that a further increase of copper, with a single plate of zinc, is also accompanied by a still further increase of power. Thus when the number of plates of copper were quadrupled and connected, the power of the combination was greatly augmented. It appeared, from the few trials made, to be principally limited by the increased distance from the zinc plate at which the external plates of copper were necessarily placed.

16. With a view of investigating the relation of the conducting power to the quantity of metal, as also the conducting powers of different metals, I resorted to an arrangement similar to that represented in Fig. 3, in which the given metal *mn*, the subject of experiment was drawn into wire, and caused to transmit a given accumulation, in a cool fluid medium, such as water; thus the error arising from an elevation of temperature was, as far as possible, avoided (12), the water itself being inadequate to conduct any portion of the charge (9). The battery employed in these experiments was suspended by balance-weights in a convenient frame, Fig. 4, so as to remove it at pleasure from the acid, the latter being contained in a vessel of wood, having an external glass-tube *rt*, and a graduated scale, by which the quantity of metal immersed within could be correctly ascertained.

17. Previously to examining the indications on the electrometer E, Fig. 3, with a given wire *mn*, the force of the battery

was correctly determined by making the circuit with the large copper wires *pm*, *qn*, through the electrometer E alone, or otherwise through a second electrometer set apart for that particular purpose. The wire *mn* being now included in the circuit, the effect on E was again examined, and, finally, the force of the battery again observed as before; any error, therefore, which might arise from a decrease in the power could be detected and corrected by the addition of a small quantity of acid, or by bringing a further portion of the battery into action; the variations, however, in this respect, for a short series of experiments, were inconsiderable, and very easily rectified. The wire in the electrometer E was about the $\frac{1}{80}$th of an inch in diameter, and was such that the indications on the scale corresponded very nearly with the increased power when successive and equal portions of the battery were immersed. The following are a few of the many interesting facts which seemed to be elicited by the method of investigation.

(*i*) When four wires, whose diameters varied from the $\frac{1}{100}$th to the $\frac{1}{27}$th of an inch, were successively placed in the circui , as in *m n*, Fig. 3, the indications of the electrometer A with the less perfect conductors, such as platinum and lead, seemed for the most part in the ratio of these diameters: with the more perfect conductors, such as silver and copper, the wires of the lesser diameters only corresponded to the same ratio, whilst, on the contrary, the smallest wires of the inferior conductors seemed, as already shewn (7) to be in a great degree inaccessible to the influence of the action. The following Table contains the results of some experiments with a given charge on copper, platinum, and lead, three metals which appear to differ materially in their conducting power.

19. This order, with the exception of tin, is about the same as that already observed with the common electrical discharge *, and was invariably shewn when the wires were placed in the circuit, externally, as above described. It is, however, liable to exceptions when deduced from the heating effects observed, by including the metals in the electrometer; hence, in applying the principle arrived at by Mr Children, in the course of his fine experiments with a battery of large plates, that the heat evolved by the metal, whilst transmitting the charge, is in some inverse ratio of the conducting power, we must take into account the circumstances already noticed (10), from which it appears that the best conductors may appear occasionally to be the most heated; it is therefore only in employing charges within the limit of the transmitting power that the above principle becomes applicable. The following is an experimental illustration of this with the metals, copper and platinum, when *enclosed in the bulb* of the electrometer.

TABLE VI.

Charge of Battery.	Copper.	Platinum.
32°	27°	18°
12°	6°	8°

Thus, when the force is great, the best conductor is the most

sure a good contact, the surfaces being clean and fair. The conducting powers were ascertained according to the method described in section 12, exp. *f.* In this case, the battery was provided with two additional contact cups; with these the forcep-wires above mentioned, were made to connect at pleasure by the intervention of cups containing mercury in which they were secured, and sustained on pillars of glass, as in *m n*, Fig. 3.

* Transactions of the Royal Society of London, for the year 1821, p. 438.

TABLE IV.

Wires.	No. 1.	No. 2.	No. 3.	No. 4.
Ratio of Diameters.	1	0.75	0.50	0.25
Copper, . . .	60°	60°	40°	15°
Platinum, . .	43	30	21	4
Lead, ...	36	27	18	2

It may be observed, by reference to the above Table, that the smallest copper-wire, No. 4, as compared with the largest, No. 1, indicated a conducting power in the ratio of its diameter. The same is observable on comparing No. 2. and No. 3. together, whilst the conducting power, indicated with the copper-wires, No. 1. and No. 2, seemed for the particular charge employed to be the same; hence these wires transmitted the force in action equally well,—the differences in the respective quantities of metal, therefore, did not become apparent. On the contrary, the wires of the less perfect conductors, platinum and lead, Nos. 1, 2, and 3, indicated a conducting power exactly in the ratio of their diameters, or very nearly so, whilst the indications with the smallest wires of these metals, No. 4, were inconsiderable.

18. (*k*) The deductions arrived at, on examining in this way the conducting power of various metals, were as follows:

1*st*, That, for certain and given small forces, the differences in the conducting powers vanish, each metal being equally efficient; a result quite consistent with that already arrived at in the preceding section (17), with the copper wires, Nos. 1. and 2.

2*d*, The differences in the conducting powers become more apparent, within a certain limit, as the force of the battery increases, the exact proportions in which the differences increase with the increased power, I could not correctly ascertain.

In the next Table are given the results of some few experiments on wires of platinum and copper, of about the $\frac{1}{30}$th of an inch in diameter.

TABLE V.

Metals.	Effects.	Force of Battery.
Copper, . .	21°	28°
Platinum, . .	11°	
Copper, . .	9°	11°
Platinum, . .	5°	
Copper, . .	5°	5°
Platinum, . .	3°	

The conducting powers of the above metals, therefore, with the decreasing force of the battery, constantly tended to a ratio of equality; and it was subsequently found, by employing more delicate electrometers, by which a much less charge became appreciable, that the indicated conducting powers were at last precisely equal.

3d, The order of succession of some of the metals, as regards their conducting power, was observed to be as follow:

Silver, copper, zinc, gold, tin, iron, platinum, lead, antimony, fl. mercury, bismuth *.

* The conducting powers of antimony and bismuth have been determined subsequently to the period at which this paper was read. These metals were cast into bars, and compared with zinc, tin, and lead, treated in a similar way,—a process suggested to me by Mr Forbes, who was so good as to honour me with a communication on this subject. Mercury, in its fluid state, was examined in a somewhat similar way, by causing it to form a fluid bar of like dimensions to the preceding, by means of a groove cut in a support of dry mahogany. The bars were each six inches long, and about the one-eighth of an inch square. They were held securely between stout forcep-wires, having compressing screws, as in *m n*, Fig. 3, so as to in-

heated; when less, then the inferior conductor is the most heated.

22. These experiments seem calculated to establish many important points connected with voltaic action, nevertheless it is always essential to remember, that, with our present imperfect apprehension of the causes of electricity and heat, an exact knowledge of every observed effect cannot be fairly expected; the results of these and similar inquiries, therefore, must be considered merely as useful approximations toward a higher degree of theoretical perfection. The instrument resorted to in prosecuting the foregoing researches, although not entirely without the limit of objection, is, nevertheless, convenient, and useful in practice, and sufficiently perfect for the purposes to which it is intended to be applied. In cases where it is thought desirable, mercury, as already observed (2), may be used for indicating the degrees of force instead of the coloured fluid: in this case, however, the tube for regulating the divisions on the scale should be much finer.

Plymouth, *May* 5. 1831.

On the Law of the Diffusion of Gases. By THOMAS GRAHAM, Esq. M.A. F.R.S.Ed. &c.

(Read December 19. 1831.*)*

IT is the object of this paper to establish with numerical exactness the following law of the Diffusion of Gases:

"The diffusion or spontaneous intermixture of two gases in contact, is effected by an interchange in position of indefinitely minute volumes of the gases, which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the Density of that gas."

These replacing volumes of the gases may be named *equivalent volumes of diffusion*, and are as follows: Air, 1; Hydrogen, 3.7947; Carbureted hydrogen, 1.3414; Water-vapour, 1.2649; Nitrogen, 1.0140; Oxygen, 0.9487; Carbonic acid, 0.8091; Chlorine, 0.6325, &c.; numbers which are inversely proportional to the square roots of the densities of these gases, being the reciprocals of the square roots of the densities, the density of air being assumed as unity.

If the two gases are separated at the outset by a screen having apertures of insensible magnitude, the interchange of "equivalent volumes of diffusion" takes place through these apertures, being effected by a force of the highest intensity;

and if the gases are of unequal density, there is a consequent accumulation on the side of the heavy gas, and loss on the side of the light gas. In the case of air, for instance, on the one side of the screen, and hydrogen gas on the other, a process of exchanging 1 measure of air for 3.7947 measures of hydrogen, through the apertures, is commenced, and continues till the gases on both sides of the screen are in a state of uniform mixture. Experiments on this principle can be made with ease and precision, as will appear in the sequel, and afford an elegant demonstration of the law.

There is a singular observation of Doebereiner, which chemists seem to have neglected as wholly inexplicable, on the escape of hydrogen gas by a fissure or crack in glass-receivers, which belongs to this subject, and from which I set out in the inquiry. Having occasion, while engaged in his researches on spongy platinum, to collect large quantities of hydrogen gas, he accidentally made use of a jar which had a slight crack or fissure in it. He was surprised to find that the water of the pneumatic trough rose into this jar one and a half inches in twelve hours, and that, after twenty-four hours, the height of the water was two inches two-thirds above the level of the water-trough. During the experiment neither the height of the barometer, nor the temperature of the place, had sensibly altered.

In other experiments, he substituted glass vessels of very different forms, tubes, bell-jars, flasks, all of which had fissures. In every one of these vessels, filled with hydrogen, the water rose, after some hours, to a certain height. On covering one of these vessels, containing hydrogen, by a receiver—or on filling the vessel with atmospheric air, oxygen or azote, instead of hydrogen—he never observed a change in the original volume of the gas. He thinks it probable that the phenomenon is due to the capillary action of the fissure, and that the hydrogen only is at-

tracted by the fissures, and escapes through them on account of the extreme smallness of its atoms *.

This explanation is rendered improbable by the circumstance, that hydrogen, of all the gases, was condensed and absorbed with greatest difficulty, and in smallest quantity, by charcoal and the other porous substances, tried by Saussure. And we have no reason to suppose that the particles of hydrogen are smaller than those of the other gases.

On repeating Doebereiner's experiment, and varying the circumstances, it appeared that hydrogen never escapes outwards by the fissure without a certain proportion of air returning inwards. In the experiment, however, as originally performed, it is evident, that, as soon as the water rises in the jar above its outer level, air will begin to be forced into the jar mechanically through the fissure, by the pressure of the atmosphere, independently of what we shall suppose enters by diffusion. But if we press down the jar of hydrogen to a certain depth in the water-trough, so that the level of the water without is kept constantly higher than the level of the water within the jar, then, on the contrary, a portion of the hydrogen will be forced out mechanically by the pressure to which the gas is subject. In the last circumstances, however, no air can enter by the fissure, and mix with the hydrogen, except by diffusion, or in exchange for hydrogen. Now, in a great number of experiments of this kind, the air which entered by diffusion amounted to between one-fifth and one-fourth of the hydrogen, which left the receiver at the same time. But when the circumstances were reversed, and the column of water allowed to rise in the jar above the level of the water-trough, the quantity of air which entered by diffusion was

* Sur l'Action capillaire des Fissures, &c. Annales de Chimie et de Physique, t. 24, pp. 332–334. 1823.

increased by a portion which entered mechanically; and varied from a third to a fourth part of the hydrogen, which escaped at the same time. The results, therefore, oscillate, as they should do, about our theoretical number. One volume air should replace 3.7947 volumes hydrogen; or the whole hydrogen, on escaping from the jar, should be replaced by little more than one-fourth of its bulk of air, and a very great contraction ensue.

But it is unnecessary to detail experiments made with the jar with the fissure, as with every precaution they were not precise, although at all times compatible with, and indeed illustrative of, the law. Thus a sensible contraction always took place in the bulk of the gaseous contents of the jar when filled with carbureted hydrogen of marshes, or with coal-gas, which, like hydrogen, are lighter than air, and ought therefore to be replaced by less than equal volumes of air. With olefiant gas and carbonic oxide, which approach closely to the density of air, no contraction was perceptible, not attributable to other causes, although the gases as usual wholly escaped. In the case of carbonic acid, which is heavier than air, a slight, but positive, expansion appeared to take place, the experiment being performed over mercury.

But the same fissure or opening never allows the process of diffusion to go on with the same degree of rapidity in two successive experiments, principally, I believe, from its size changing with variations in its condition in regard to humidity. The fissures appear to be extremely minute, for we cannot cause either air or the gas employed to flow through them mechanically, at the same rate as it passes by the agency of diffusion, without the application of considerable pressure. Artificial chinks, such as that obtained by pressing together ground glass-plates, or in phials fitted with accurately ground glass-stoppers, allow gas to pass through under the slightest pressure, and do not answer for the experiment.

The effects were made much more striking, in some respects, by the discovery that Wedgewood stoneware tubes, such as are used in furnace experiments, admit, from their porous structure, of being substituted, instead of jars with fissures. When shut at one end, as they are sometimes made, they may be managed like other cylindrical gas receivers. Those which are unglazed are most suitable; but do not answer the purpose, if either very dry or too damp, being permeable by a gas under the slightest pressure in the one case, and perfectly air-tight in the other. The following experiment illustrates the force and rapidity with which diffusion proceeds. A stoneware cylinder was entirely filled with hydrogen gas over water, and transferred to the mercurial trough: in forty minutes the mercury rose to a height of $2\frac{1}{2}$ inches in the receiver above the level of the mercury in the trough; half of the hydrogen had escaped, and had been replaced by about a third of its volume of air.

But these modes were superseded by the use of Paris-plaster as the porous intermedium.

A simple instrument, which I shall call a Diffusion-tube, was constructed as follows. A glass-tube open at both ends was selected, half an inch in diameter, and from six to fourteen inches in length. A cylinder of wood, somewhat less in diameter, was introduced into the tube, so as to occupy the whole of it, with the exception of about one-fifth of an inch at one extremity, which space was filled with a paste of Paris-plaster of the usual consistence for castes. In the course of a few minutes the plaster set, and, withdrawing the wooden cylinder, the tube formed a receiver closed with an immoveable plug of stucco. The less water employed in slaking the Paris-plaster, the more dense is the plug, and the more suitable for the purpose. In the wet state the plug is air-tight; it was therefore dried, either by exposure to the air for a day, or by placing the instrument in a temperature of 200° F. for a few hours; and thereafter was per-

meable by gases, even in the most humid atmosphere, if not positively wetted. The tube was finally graduated by means of mercury into hundredths of a cubic inch, and the notation, as is usual with gas-receivers, counted from the top.

When such a diffusion-tube, six inches in length, was filled with hydrogen over mercury, the diffusion, or exchange of air for hydrogen, instantly commenced, through the minute pores of the stucco, and proceeded with so much force and rapidity, that within three minutes the mercury attained a height in the receiver of upwards of two inches above its level in the trough. Within twenty minutes the whole of the hydrogen had escaped.

In conducting such experiments over water, it was necessary to avoid wetting the plug. With this view, before filling the diffusion-tube with hydrogen, the air was withdrawn by placing the tube upon the short limb of an empty syphon (see figure), which did not reach, but came within half an inch of the plug, and then sinking the instrument in the water-trough, so that the air escaped by the syphon, with the exception of a small measure, which was noted. The diffusion-tube was then filled up, either entirely, or to a certain extent, with the gas to be diffused.

The ascent of the water in the tube, when hydrogen is diffused, forms a striking experiment. In a diffusion-tube fourteen inches long, the water rises six or eight inches in as many minutes. The column of water attains in a short time its maximum height, at which, however, it is never long sustained; for as in Doebereiner's experiment, air is all along entering mechanically through the porous plug in such circumstances, from the pressure of the atmosphere; and after the diffusion is over, the water subsides, in the course of several hours, to the general level. In

experiments made with the purpose of determining the proportion between the gas diffused and the return-air, it was therefore necessary to guard against any inequality of pressure, which was managed much more easily when the tube was standing over water than over mercury.

The capacity of a mass of stucco to absorb and condense in its pores the various gases, was made the subject of experiment, as this property might interfere with the results of diffusion. The mass was previously dried at 200° F. It absorbed at the temperature of the atmosphere, which at the time was 78°.

6.5	volumes	ammoniacal gas,
0.75	,,	sulphurous acid gas,
0.5	,,	cyanogen,
0.45	,,	sulphureted hydrogen,
0.25	,,	carbonic acid.

Oxygen, hydrogen, nitrogen, carbonic oxide, olefiant gas, coal-gas, were not absorbed in a sensible proportion, even when the temperature was 58°. It is evident, therefore, that the absorbent power which stucco enjoys, as a porous substance, is inconsiderable. Placed in humid air, the same mass of stucco absorbed 1½ per cent. of hygrometric moisture. In setting, 100 parts of the stucco had retained 26 parts water uncombined, which escaped on drying at a moderate temperature, so as to avoid decomposing the hydrated sulphate of lime. It can be shewn from this, that the vacuities must have amounted to one-third of the volume of the mass.

I shall treat in succession of the escape of the different gases from a diffusion instrument into air. As the contained gas bears no proportion in quantity to the external air, the gas escapes entirely, and is wholly replaced by air. It is of the utmost importance to determine the proportion between the volume of gas diffused, and the replacing volume of air eventually found in the

instrument. We thus obtain the *equivalent diffusion-volume* of the gas, which it will be convenient to state in numbers, with reference to the replacing volume of air as unity. I shall begin with hydrogen gas, although attended with peculiar difficulties, as it introduces in a distinct manner to our notice several circumstances which may slightly modify the results of diffusion.

1. *Diffusion-volume of Hydrogen Gas.*

I shall in this paper adopt the specific gravities of the gases generally received in this country. Of hydrogen the specific gravity is 0.0694 (air = 1), of which number the square root is 0.2635. Now, according to our law, 1 volume hydrogen should be replaced by 0.2635 air. But to have the replacing volume of air = 1,

$$0.2635 : 1 :: 1 : 3.7947;$$

or, $\frac{1}{0.2635} = 3.7947$; that is, 1 air should replace 3.7947 hydrogen. With the specific gravity of hydrogen adopted by Berzelius, namely, 0.06885, the equivalent diffusion-volume of hydrogen is 3.8149.

In a diffusion-tube standing over water, temperature 65°, 88 volumes hydrogen were replaced by 26 air; 84 hydrogen by 25 air; and in another tube, 130 hydrogen by 38 air. The quantity of return-air is here related to the hydrogen diffused, as 1 to 3.38, 3.36, and 3.42, numbers which approach to, but fall short of, the theoretical diffusion-volume of hydrogen, namely, 3.79. But the hydrogen in these experiments was saturated with vapour at 65°, which would make its density 0.0809, and reduce its diffusion-volume to 3.5161; while the air without, being comparatively dry, would be somewhat expanded *after* it entered the diffusion-tube, by the ascent of vapour into it. This would occasion the quantity of return-air to appear greater than

it should be; but it is difficult to find elements for a proper correction, as not only the quantity of vapour in the atmosphere must be taken into account, but also the hygrometric state of the plug itself. The increased return-air, however, evidently lowers the diffusion-volume of the hydrogen gas.

With the view of increasing the capacity of the instrument, and the number of its divisions, and of obviating the interference of vapour, the mode of performing the experiment was varied. On a tube, four-tenths of an inch in diameter, a bulb of two inches in diameter was blown, as in figures A and B. The tube above and below the bulb, in the case of A, was graduated into two-hundredths of a cubic inch. The upper end of the tube was closed by stucco, as in the case of the simple diffusion-tube. The general mode of proceeding will be best conceived from the recital of the details of a particular experiment.

The diffusion-instrument employed in the following experiment contained 855 measures, and was of the form A. The stucco plug was unusually large, being 0.6 inch in length, which occasioned the diffusion to be slow. At the commencement of the experiment the thermometer stood at 68°, and the barometer 29.73 inches. The bulb being sunk in water with the air-syphon in it, the whole air was withdrawn, with the exception of 12 measures, and the instrument filled up with newly made hydrogen gas. So that at the outset we had in the instrument,

Air with its vapour, ...	12
Hydrogen, ...	823.83
Vapour (accompanying the hydrogen at 68°),	19.17
	855.00

As soon as it was filled, it was placed in a glass-jar, of about the same height, with a little water left in the bottom, and in proportion as the water rose in the tube of A, from the subsequent contraction, the jar was filled up by repeated additions of water, so as to keep the surface of the water, within and without the tube, as nearly as possible at the same level. With the view of having the external air in a constant state in regard to humidity, means were taken to saturate it. A small cone of damp paper was inverted, like an extinguisher, over the upper part of the instrument; the jar containing the instrument was placed on the shelf of the pneumatic trough, and a bell-jar with an opening at the top, which could be shut at pleasure, inverted over the whole. The return-air must therefore have been in the same state, in regard to humidity, as the hydrogen itself. Aqueous vapour would diffuse neither outwards nor inwards, as it existed in the same proportion on both sides of the plug; but dry hydrogen only would be exchanged for dry air, in the proportion of their equivalent diffusion-volumes.

In the first thirty-four minutes, the gaseous contents of the bulb were diminished by 95 measures, and ultimately, in twenty-six and a half hours, they were reduced to 227 measures, which were common air. The contraction in this and other cases, in which the water rose into the bulb, was determined by weighing, at the end of the experiment, the water which had entered; a mode which admits of even greater nicety than measuring the bulk of residuary gas in a graduated vessel.

With the view of obtaining elements for a correction for any change in the bulk of the gas, which might take place during the continuance of the experiment, from changes in temperature, pressure, or from solution of the gas in water, a receiver was made of the same tube, with a bulb of nearly the same capacity as the diffusion instrument, but close at the top.

This receiver was also nearly filled at the commencement of the experiment with hydrogen gas, and the quantity of gas noted, the tube being graduated. The hydrogen in this standard receiver contracted $\frac{1}{82}$d part during the experiment. We have therefore to increase the quantity of air found ultimately in the diffusion-receiver by $\frac{1}{82}$d part. In this way the residuary air is increased to 229.8 measures, 12 of which, or, more correctly, 11.85 ($= 12 - \frac{1}{82}(12)$), were present from the beginning. The temperature was also 68° at the end of the experiment, the same as at the beginning. The ultimate contents of the diffusion-instrument may be stated with sufficient accuracy as follows:—

Air and vapour originally present,	11.85
Dry air which has entered,	212.84
Vapour in do.	5.11
	229.80

The conclusion is, that 823.83 measures dry hydrogen have been replaced by 212.84 dry air. Now,

$$\frac{823.83}{212.84} = 3.87 = \text{diffusion-volume of hydrogen.}$$

The diffusion-volume of hydrogen comes out above the theoretical number in this experiment, but an addition of not more than 2 per cent. to the quantity of return-air, would reduce it below the theoretical number. The quantity of vapour which was supported by the hydrogen at the commencement of the experiment was 19.17 measures, but at the end of the experiment we find only 5.11 measures vapour; the difference has condensed, from the loss of a permanently elastic fluid necessary to support it.

As the quantity of hydrogen and of return-air is amplified in the same proportion by vapour, provided the temperature be the same at the beginning and end of the experiment, it is unne-

cessary to know the absolute quantity of vapour in either case, in determining the diffusion-volume of hydrogen. We may simply divide the gross amount of hydrogen gas diffused, by the gross amount of return air, the quotient is the diffusion-volume of hydrogen.

Experiment 2.—The thickness of the stucco-plug in the instrument used above, was reduced from six-tenths to two-tenths of an inch, by cutting away the upper portion. The instrument, of the same capacity as before, was now entirely filled with hydrogen gas. This was effected, by first filling up with hydrogen, leaving a small quantity of air in the upper part of the instrument as in the previous experiment, then withdrawing this impure hydrogen by the air-syphon, and filling up a second or third time with the same gas, whereupon the proportion of air remaining ceased to be appreciable. The apertures of the plug were closed, by pressing the finger upon its upper surface; and in this manner any diffusion of the hydrogen was carefully guarded against, till the process of filling was completed. The diffusion was so rapid in the case of the thin plug, that this additional precaution was absolutely required. Care was taken to have the return-air saturated with moisture in this and every other experiment of the same kind, and inequality of pressure was avoided.

At the beginning of the experiment, the instrument contained 855 measures hydrogen, saturated with vapour at 62°; in three minutes a contraction of 95 measures took place, and in the course of an hour the diffusion was sensibly at an end. The instrument, however, was exposed for two hours longer, that the diffusion might certainly be complete. During intervals so short uniformity of temperature might be counted upon, with certain precautions; and the variations in atmospheric pressure were

generally so minute, that they might be neglected with impunity. Corrections for temperature and pressure might therefore be dispensed with, which was a great advantage. 855 measures hydrogen were found eventually to be replaced by 226.5 measures air, both saturated with vapour at 62°.

$\frac{855}{226.5} = 3.774 =$ diffusion-volume of hydrogen. This determination is somewhat below the theoretical diffusion-volume, 3.79, while the preceding determination was in excess.

Experiment 3.—Another diffusion-instrument of the form B, with a dense plug, one-tenth of an inch in thickness, was filled with water, which was then poured into a counterpoised phial, and found to weigh 1085.7 grains. When filled over water, 1085.7 grain-measures of gas are therefore introduced into this instrument, and in this way we express most correctly its capacity. The instrument, after the plug was dried, was entirely filled with hydrogen gas, as in the preceding experiment, thermometer 61°. The bulk of the diffusion appeared to be over in an hour and a half, but five hours were allowed to the experiment. Thereafter the water which had entered the instrument was poured into a counterpoised phial, and found to weigh 800.6 grains. This last quantity represents the contraction, and subtracting it from 1085.7, we have the return-air equal to 285.1 grain measures. Now,

$\frac{1085.7}{285.1} = 3.808 =$ diffusion-volume of hydrogen gas.

Experiment 4.—Same bulb, circumstances the same, but thermometer 62°. Time allowed for the diffusion four hours.

1085.7 measures hydrogen were replaced by 286.1 measures air.

$\frac{1085.7}{286.1} = 3.795 =$ diffusion-volume of hydrogen.

Experiment 5.—Same bulb, &c. thermometer 61°. Time five hours.

1085.7 measures hydrogen were replaced by 278.4 measures air.

$\frac{1085.7}{278.4} = 3.900 =$ diffusion-volume of hydrogen.

Experiment 6.—Same bulb, but in this and the succeeding experiment, the bulb was attached to the end of a balance, and counterpoised, so that it adjusted itself spontaneously in the jar filled with water, in which it floated. Thermometer 60°.

1085.7 measures hydrogen were replaced by 279.1 measures air.

$\frac{1085.7}{279.1} = 3.890 =$ diffusion-volume of hydrogen.

Experiment 7.—Same repeated. Themometer 61°.

1085.7 measures hydrogen were replaced by 282.2 measures air.

$\frac{1085.7}{282.2} = 3.847 =$ diffusion-volume of hydrogen.

The results of these five last experiments, with the same instrument, are, in one view,

Measures of Return Air.	Diffusion-volume of Hydrogen.
285.1	3.808
286.1	3.795
278.4	3.900
279.1	3.890
282.2	3.847
Mean, 282.2	Mean, 3.848

New hydrogen gas was made for each experiment by the moderate action of dilute sulphuric acid on zinc, and it was collected in the diffusion-instrument from the beak of the retort. The observations could not be made with so much accuracy as to entitle us to place any reliance on more than two decimal places of the calculated diffusion-volumes. A great variety of experiments were performed on the diffusion of hydrogen with the diffusion-bulbs employed above, and several others of similar construction, principally with the view of discovering the cause of the slight variations in the results, and why the quantity of return-air was pretty uniformly somewhat less than the theoretical quantity, which has the effect of increasing the proportion of the hydrogen diffusion-volume.

It appears, that when the stucco-plug is in a parched state, the quantity of return air is uniformly greater than it should be. Thus 3.65 and 3.69 were the diffusion-volumes of hydrogen deduced from an experiment, in the one case with a plug which had been dried at 100°, and subsequently exposed for several hours to the air, and in the other case, with a plug merely dried in air, temperature 68°. The obvious cause of this is, that the air is dried in passing through the plug, and is subsequently expanded while in the diffusion-instrument by the ascent of vapour into it. Hence, the first time a diffusion-bulb is tried, it generally gives the diffusion-volume of hydrogen below the truth.

On the other hand, I apprehend, that when the pores of the stucco are saturated with hygrometric moisture, which, from the circumstances of the experiments, must be almost always the case, the hydrogen, in making its way through the plug, actually avails itself to a small extent of this moisture, inducing it to vaporize, and exchanging places with it instead of air. Hydrogen which escapes in this way will not be represented by return-air, the quantity of which is thus diminished. This process, however, is extremely intricate, and has not yet been fully investigated. Its

effect is insensible in the case of the other gases, of which the diffusion-volumes approach more closely to that of air.

The more dense and compact the plaster-plug, the more correct appear to be its general indications. On this account I compress the plug, while moist, before it sets. When the plug is of a loose structure, and probably contains sensible vacuities in its substance, diffusion goes on with increased rapidity; but I have observed, that the proportion of return-air is notably diminished in the case of the diffusion of hydrogen. Thus, in a set of experiments with a diffusion-bulb, having a plug of this description, and little more than one-tenth of an inch in thickness, I obtained, as the diffusion-volume of hydrogen, 4.05, 4.04, and 4.00. This plug had been somewhat thicker at one time, and then gave 3.93 as the diffusion-volume of hydrogen. These experiments exhibit an extreme case of this deviation. It appears to depend upon some physical property of hydrogen gas which is peculiar to it. To obtain light upon this subject, I was led to investigate the rate at which air, hydrogen, and the other gases flow through the stucco-plug into a vacuum, under the influence of mechanical pressure.

A small bell-jar, with an opening at top, was used, which opening was closed with a plug of Paris plaster of half-an-inch in thickness, over which a brass cap and stopcock were fitted and cemented. This receiver was placed on the plate of an air-pump in perfect order, and exhausted. When the stopcock of the receiver was closed, nothing entered the exhausted receiver; but on opening it, either air entered, forcing its way through the pores of the stucco, or any gas which might be conducted to it, by means of a flexible tube from a proper magazine.

The time was noted in which the mercury of the gauge-barometer, in communication with the receiver, fell two inches, always setting out with gas of the tension of one inch mercury in

the receiver, and stopping exactly when it attained a tension of three inches.

Air entered, according to eight or ten experiments made on different days, in within ten seconds, more or less, of ten minutes, and so whether the air was saturated with aqueous vapour or dry.

The same volume of different gases entered in the times expressed in the following table, under the same pressure, or beginning at a pressure of 29 inches mercury, and terminating with a pressure of 27 inches:

	Minutes.		Seconds.
Air, dry, - - - -	10	...	0
Air, saturated with moisture at 60°, -	10	...	0
Carbonic acid, - - -	10	...	0
Nitrogen, - - - -	10	...	0
Oxygen, - - - -	10	...	0
Carbonic oxide, - - -	9	...	30
Olefiant gas, - - - -	7	...	50
Coal gas, - - - -	7	...	0
Hydrogen, - - - -	4	...	0

In repetitions of the experiments, the numbers oscillated 10, or 12, sometimes 20 seconds, on either side of the numbers given in the table, from circumstances which could not easily be appreciated. As the mercury in the gauge fell not continuously, but by leaps, from adhesion to the glass, the experiments are not susceptible of the greatest accuracy.

The greater the pressure the more rapidly are gases forced through the pores of the plug; but the quantity of gas which penetrates in any given time is not exactly proportional to the pressure, at least in the case of air and hydrogen. By doubling the pressure, we do not quite so much as double the quantity of gas forced through; or a fixed quantity of gas does not enter in

half time under double pressure, as will be evident from the following table of observations. Pressure of atmosphere 30 inches.

Height of Gauge Barometer in inches of mercury, or Pressure.	Air. Interval of time in falling one inch by gauge.		Hydrogen. Interval of time in falling one inch by gauge.	
	Minutes.	Seconds.	Minutes.	Seconds.
29	0	0	0	0
28	5	0	1	50
27	5	23	2	0
26	5	15	1	55
25	5	30	1	55
24	5	35	2	0
23	5	45	2	2
22	6	0	2	13
21	6	5	2	10
20	6	30	2	35
19	6	35	2	30
18	7	3	2	40
17	7	12	2	50
16	7	35	3	10
15	8	10	3	30
14	8	40	3	35
13	9	10	4	5
12	9	55	4	10
11	11	0	4	15
10	11	40	4	30
9	12	30	5	20
8	14	15	7	40

The ratio of the times, in hydrogen and air, is not greatly different at different pressures. Thus, the mercurial column was depressed 18 inches, or from 29 to 11 inches.

By Air, in - 7283 seconds,
By Hydrogen, in 3025 seconds,

$$\frac{7348}{3025} = 2.408 = \text{Ratio of hydrogen,}$$
$$1. \quad = \text{Rate of air.}$$

It was found that the kind of gas in the receiver made no dif-

ference on the velocity with which hydrogen entered under a certain pressure. Hydrogen entered as rapidly against hydrogen in the receiver of a certain tension, as against air of the same tension. Thus,

Barometer Gauge. Height.	Hydrogen entered against Hydrogen, (*From preceding Table.*)		Hydrogen entered against Air.	
	Time.		Time.	
Inches.	Minutes.	Seconds.	Minutes.	Seconds.
15	0	0	0	0
14	3	37	3	35
13	3	56	4	5

It is evident from this, that the air does not diffuse out against so strong a pressure and the inward current of hydrogen.

When this jar, of which the capacity was 65 cubic inches, was used as a diffusion-instrument, and filled over water with hydrogen, one-fourth of the hydrogen which it contained escaped by diffusion into air in the first hour. Now, we find by the Table, (p. 18.), that hydrogen penetrates the plug with greater velocity when passing into a vacuum or into the exhausted receiver. The exhausted receiver was filled one-fourth in about fifteen minutes; hence a certain quantity of hydrogen passed through the same porous plug, by the pressure of the atmosphere, into a vacuum in fifteen minutes; by spontaneous diffusion into air in sixty minutes; or the velocity of diffusion was one-fourth the velocity of mechanical pressure.

This was a dense and excellent plug; and in others of a looser texture, the velocity of diffusion was much less than a fourth.

Dried bladder answers for shewing the diffusion of hydrogen when stretched over the open end of the tube receiver. The diffusion, however, through a single thickness of bladder, is ef-

fected at least twenty times more slowly than through a thickness of one inch of stucco. While, on the other hand, either air or hydrogen, under mechanical pressure, passes more readily through bladder than a great thickness of stucco. Goldbeaters skin is even more permeable by gases under a slight pressure than bladder, and less suitable for diffusion.

The superior aptitude of stucco for exhibiting the unequal diffusion of gases of different densities, seems to depend upon its pores being excessively numerous, but exceedingly minute, making in the aggregate a considerable channel. In the bladder, or goldbeaters' skin, the pores I suppose to be few in number but wide, making, however, when added together, but a small channel. Air passes through them but little impeded by friction.

Dry and sound cork answers exceedingly well as a substitute for the stucco-plug. The diffusion takes place slowly, but is not apt to be deranged by a slight mechanical pressure. So do thin laminæ of many granular minerals, such as the flexible magnesian limestone, &c.; charcoal also, and woods, if not too porous, may be applied to the purpose.

It might occur, in explanation of our experiments with the diffusion-instrument, to take Mr Dalton's hypothesis, and suppose, in the case of hydrogen, the external air to be a vacuum to the hydrogen, and the hydrogen a vacuum to the air, and that the *inequality* of the diffusion depends upon the hydrogen *being least resisted in passing through the plug.* The experiments on the permeability of the stucco by gases under pressure, above detailed, were projected with a view to settle this point among others; and they are evidently incompatible with such an application of the theory, for hydrogen passes 2.4 times more swiftly, and not 3.8 times, as in the diffusion experiments. Carbonic acid, too, permeates the plug, under pressure, as rapidly as air does, or even somewhat more rapidly, for our results inclined to this side rather than to the other; whereas carbonic acid diffuses through

the plug more slowly than air does, or is replaced by more than an equal volume of air, as will presently appear.

Those experiments, previously narrated, are perhaps sufficient to establish the law in regard to hydrogen, particularly when we find it hold in the case of other gases.

As hydrogen is a very light gas, I was anxious to establish the law also in regard to a heavy gas, such as carbonic acid.

2. *Diffusion of Carbonic Acid Gas.*

The most satisfactory experiments with carbonic acid gas were performed by confining it over a solution of common salt, saturated in the cold, which absorbs this gas very slowly, and, instead of the diffusion-instrument with bulb, a long diffusion-tube was found most suitable.

Experiment 1.—Thermometer 64°; dew-point 53°. Barometer 30.13. Left in diffusion-tube 17 air, and filled up over brine to 197 with carbonic acid gas, which gives 180 carbonic acid. As brine boils at 222° or 224°, that is 11° or 12° above the boiling point of water, we may suppose it to be proportionally less vaporous at low temperatures, and take the tension of its vapour at 64° to be that of water at 53°, which was also the dew point. This was confirmed by confining 847 volumes of atmospheric air over brine at the time; the air was not expanded by vapour rising into it from the brine, nor did it contract.

The initial contents of the diffusion are therefore,

Air and vapour, - - -	17.
Carbonic acid gas, - - -	177.6
Vapour, - - - -	2.4
	197.0

An expansion took place of 4 measures in ten minutes, and of 40 measures in five hours. A standard tube of the same diameter as the diffusion-tube, sealed at the top, had been filled with carbonic acid and placed over brine, to mark the absorption of the gas. One measure of gas was absorbed during the continuance of the above experiment. The expansion, therefore, in the diffusion-case has really been 41 and not 40, or, probably even more than 41, as undoubtedly a greater absorption of gas by the brine occurred in the diffusion-tube than in the standard-tube, from the motion of the liquid in the former during the course of the expansion of its gaseous contents, while the liquid in the other was quite at rest, and 177.6 — 1 ,or 176.6 carbonic acid gas only have been exposed to diffusion. The diffusion was allowed to take place into the open air, which had the same proportion of vapour as the carbonic acid.

The specific gravity of carbonic acid gas is 1.527, of which the square root is 1.2360, and the reciprocal of the square-root 0.8091. Hence one volume air should replace 0.8091 carbonic acid gas, which is the theoretical diffusion-volume of this gas.

In the experiment, 176.6 carbonic acid are replaced by 217.6 air.

Here, the expansion upon 176.6 carbonic acid being replaced by air is 41 + parts by experiment, while it is 41.68 parts by theory.

The diffusion-volume of carbonic acid gas is,

0.812 by experiment,

0.809 by theory.

Experiment 2.—In another experiment, conducted in the same manner, thermometer 64°, barometer 30.00, the initial contents of the diffusion-tube were,

Carbonic acid and vapour, 201.

The final contents,

Air and vapour, - - 245.

Correcting for loss of gas by absorption, the final contents would be,

Air and vapour, 246.

As the proportion of vapour in the gas at the first, and in the air finally is the same, we may say that carbonic acid is replaced by air in the proportion of 201 to 246.

$\frac{201}{246} = 0.813 =$ diffusion-volume of carbonic acid.

Experiment 3.—In a third experiment over brine, thermometer 62°, barometer 29.65, carbonic acid and vapour, . 169

Replaced by air and vapour, ... 205

Or, allowing for absorption, by air and vapour, . . 206

$\frac{169}{206} = 0.816 =$ diffusion-volume of carbonic acid.

But extreme accuracy is quite out of the question in the case of carbonic acid, from the vagueness of the small correction for absorption of the gas by the brine, and from the absorbent action of the plug, which affects, more or less, all the condensible gases.

The experiment in the case of this gas had been performed repeatedly over water itself, in different diffusion-tubes, and always with an eventual increase to the gaseous contents of the tube of within 2 per cent. of the theoretical quantity, but this mode, and the corrections for absorption, are decidedly inferior in precision to the preceding.

3. *Chlorine.*—This gas, from its high density, should afford a good illustration of the law, were other circumstances equally favourable, as the specific gravity of chlorine is about 2.5, of which the square-root is 1.5811, and the reciprocal of the square-root 0.6325. 100 measures of chlorine should be replaced by 158.11 air; or 1 air should replace 0.6325 chlorine, which is its diffusion-volume.

Experiment.—Thermometer 64°. To a diffusion-tube over water, with 5 measures air, 80 chlorine gas were added, making together 85 measures, which, diffusing into damp air, expanded 3 measures in the first eight minutes, 18 measures in eighty-two minutes, and, finally, 19 measures in one hundred and six minutes; but the same gas, in a close standard tube of the same diameter, contracted, owing to absorption of the gas by water, 5 measures in eight minutes, 15 measures in thirty-three minutes, and 18 measures in thirty-nine minutes, the rate of absorption diminishing evidently from the water in the tube becoming saturated and abiding in it. But the absorption of gas by water in the two experiments cannot be well compared, for, in the diffusion experiment, the chlorine is rapidly diluted with return-air, which protects it from absorption, and, indeed, before the end of the experiment, must occasion a portion of the dissolved chlorine gas to reassume the gaseous form, vaporizing away from the water which held it in solution, and rising into the upper part of the tube. The absorption in the diffusion-case would certainly be overrated at one-half of what occurred in the comparative experiment in the same time. At the outset, however, we may presume that the same absorption took place in both cases. Hence the expansion in the diffusion experiment would be 3 + 5, or 8 measures to the first eight minutes. The absorption, however, would tell two ways in lessening the expansion; *first*, so much gas has disappeared by absorption, the quantity to be added to the expansion; *second*, so much less chlorine has really been submitted to diffusion: 80 parts have not been diffused, but 80 diminished by this quantity.

Merely adding the observed absorption in the first thirty-nine minutes, namely 18 measures to the expansion observed of 19 measures, we have an expansion from diffusion of 37 measures, which approaches, as near as we can expect from the method, to 45 measures, the theoretical expansion on 78 measures dry chlo-

rine. We may therefore presume that the diffusion of chlorine is not incompatible with the law.

4. *Sulphurous Acid Gas.*—Over mercury. To diffusion-tube with 7 measures air, 66 dry sulphurous acid gas were added, which were allowed to diffuse into dry air. An expansion occurred of

5 measures in 9 minutes,
13 23
30 85
31 108

at which last expansion it remained steady.

Assuming the specific gravity of sulphurous gas at 2.222, its square-root is 1.4907, of which the reciprocal is 0.6708.

67.08 sulphurous gas should be replaced by 100 air.

We have 66 sulphurous gas, and expansion 31, or,

66 sulphurous acid are replaced by 97.00 air, by experiment;
66 .... 98.39 air, by theory.

The diffusion-volume of sulphurous acid gas is,

0.68 by experiment,
0.67 by theory.

5. *Protoxide of Nitrogen.*—In an experiment with this gas, dry, over mercury, allowing for a quantity of nitrogen which it contained, 51 measures were replaced in ninety minutes by 62 dry air. Taking the specific gravity of this gas at 1.2577, its root is 1.2360, of which the reciprocal is 0.8091.

Diffusion-volume 0.82 by experiment,
.... 0.81 by theory.

6. *Cyanogen.*—Also over mercury. First deprived of hydrocyanic acid by peroxide of mercury, and dried, an expansion always resulted from diffusion, but it never amounted to the theoretical quantity. Taking 1.8105 as the specific gravity of cyano-

gen, the square-root is 1.3456, and the reciprocal of the square-root 0.7432.

Hence, 1 cyanogen is replaced 1.3456 air; and
1 air replaces . . 0.7432 cyanogen.

1*st*, 83 cyanogen were replaced by $99\frac{1}{2}$ air; 2*d*, 75 cyanogen by 90 air; 3*d*, 50 cyanogen by 63 air. The last experiment is the most favourable. But 100 cyanogen are replaced, according to that experiment, by 126 air only, instead of 134. This deviation from the law, depends on the property of the plaster-plug, which it shares with all porous bodies, to absorb and condense a portion of all those gases which, like cyanogen, are easily liquefied. It is evident, that if a portion of the cyanogen is withdrawn in this way, a certain contraction is occasioned, and again really less of the gas is submitted to diffusion, and from both causes, the expansion is less than it ought to be. It is possible, also, that the cyanogen may have contained a little nitrogen.

7. *Muriatic Acid Gas.*—Specific gravity 1.28472; square root, 1.1334; reciprocal of square root 0.8823. Hence,
1 muriatic acid should be replaced by 1.2847 air; and
1 air should replace 0.8823 muriatic acid.

In the case of this gas, the expansion from diffusion was overpowered by the absorbent property of the plug.

94 measures contracted to 88 in ten minutes, and remained at that quantity for nine minutes, and then expanded to 90 measures in twenty-five minutes more. The plug, upon a subsequent examination, appeared to be injured, and rendered too permeable, by a chemical action of the muriatic acid upon the hydrated sulphate of lime.

8. *Ammoniacal Gas.*—Density 0.5902. Square root 0.76825; reciprocal of square root 1.3016. Hence,
1 ammoniacal gas should be replaced by 0.76825 air; and
1 air should replace 1.3016 ammoniacal gas.

But in the case of this gas, as with muriatic acid, the result of diffusion is altogether deranged by condensation of gas in the porous plug, which, in these experiments, was half an inch in thickness. It is remarkable, however, that, when the tube was filled with ammoniacal gas in the usual way, the final contraction was by no means excessive, indeed, never quite so great as it should have been from diffusion alone, independently of the contraction from absorption. This was found to arise from the absorption by the plug being so rapid, that, during the progress of filling the tube with gas, the plug became nearly saturated with gas, taking up ten or twelve times its bulk, and, consequently, a great deal more gas was introduced into the tube than its capacity.

9. *Sulphureted Hydrogen Gas.*—Prepared from sulphuret of antimony, by the action of muriatic acid. Density, 1.1805. Root, 1.0855. Reciprocal of root, 0.9204.

In the case of this gas, 69 measures were replaced by 73 air. In this experiment, 100 air replaced 95 instead of 92 sulphureted hydrogen. But we may refer the diminution to the absorption of the gas by the plug, and to its partial decomposition, as the mercury exposed to the gas became black. The air which entered contributed to this decomposition.

As carbonic acid is one of the gases condensed by the plug, like the preceding examples, but to a less extent, we can now understand why the return air was always a little under the theoretical quantity, in the careful experiments on that gas, of which an account was formerly given.

In the case of the gases which follow, the specific gravity approaches so closely to that of air, that their accordance with the law requires every precaution.

10. *Oxygen Gas.*—Specific gravity, 1.111. Square root, 1.0541. Reciprocal, 0.9487.

100 oxygen should be replaced by 105.41 air; and
100 air should replace 94.87 oxygen.

When confined in a straight diffusion tube, there is uniformly an expansion; but it is unnecessary to recount experiments performed with the straight tube, as the divisions are not minute.

Experiment 1.—Thermometer 64°. Barometer 29.82 inches. Diffusion-instrument with bulb, divided into two-hundredths of a cubic inch; also standard bulb and tube, close at top, to afford corrections for changes in temperature and pressure, as before explained. Both diffusion-instrument and standard were filled with pure oxygen from chlorate of potash, and placed in glasses over water, covered by a bell-jar, of which the inside was moistened. A few minutes were purposely allowed to elapse before the quantity of gas in either instrument was noted, as the quantity oscillated for a little. The diffusion-instrument contained 795 measures oxygen, and the standard 828, at the outset. In two hours the expansion in diffusion-instrument, corrected from the standard, was 6 measures; in four hours and a half, 13 measures; in fifteen hours, 29 measures; in twenty hours, 34 measures; in twenty-nine hours, 41 measures; in thirty-eight hours, the expansion was at a maximum, namely, 43 measures. In explanation of the long duration of this and the following experiments, it may be stated, that the plug was fully half an inch in thickness.

795 measures oxygen and vapour have therefore been replaced by 838 measures air and vapour.

$\frac{795}{838} = 0.9487 =$ diffusion volume of oxygen by experiment.

This is the exact theoretic number, a coincidence, however, which we must view as accidental.

Experiment 2.—In a careful repetition of this experiment with another specimen of oxygen gas, the results approached very

closely to the preceding; but the return air was in slight excess above the theoretical quantity. Thus,

1 oxygen was replaced by 1.056 air, by experiment.

1 .... 1.054 air, by theory.

Oxygen, therefore, affords a most striking confirmation of the law.

11. *Nitrogen.*—Prepared by burning an excess of phosphorus in a confined portion of air, and allowing the residuary gas to stand over water for several days.

Specific gravity, 0.9722. Root, 0.9860. Reciprocal of root, 1.0140. 100 nitrogen should be replaced by 98.60; and 100 air should replace 101.40 nitrogen.

Thermometer, 66°. Barometer, 29.23. Diffusion into moist air as in the preceding experiments.

836 measures contracted 3 measures in two hours and forty minutes, as corrected by standard; and 13 measures in eighteen hours, which was the maximum contraction; for in twenty-three hours and a half from the beginning of experiment, a contraction of 12 measures was indicated. Taking the last as the true result,

$\frac{836}{834} = 1.0143 =$ diffusion-volume of nitrogen by experiment.

$1.0140 =$ diffusion-volume of nitrogen by theory.

12. *Olefiant Gas.*—Specific gravity likewise 0.972, &c. as in nitrogen. The gas was carefully made, collected in a low receiver, allowed to stand over water for twenty-four hours, and finally washed with caustic ley.

Thermometer, 59°. Barometer, 29.83. 800 measures of this gas were replaced by 785 measures of air, in twenty-five hours, correcting from standard.

$\frac{800}{785} = 1.0191 =$ diffusion-volume of olefiant gas, by experiment.

The contraction in this experiment is a little above the theo-

retical quantity. In another experiment with different gas, the contraction was even greater, indicating a diffusion-volume = 1.0303; but the presence of a minute quantity of carbureted hydrogen, or some lighter hydro-carburet, was suspected, from the rapidity of the contraction in this case.

13. *Carbonic Oxide.*—Specific gravity, 0.9722, &c. as in the case of nitrogen. Gas prepared by the action of sulphuric acid on crystallized oxalic acid, well washed with caustic ley.

On 803 measures carbonic oxide and vapour, a contraction of 11 measures in fifty hours, 12 measures in eighty-nine hours, 12 measures in ninety-seven hours; or 803 became 791. The diffusion was slower than usual, from the plug having been partially wetted in filling the instrument with gas.

$\frac{815}{803} = 1.0149 =$ diffusion-volume carbonic oxide, by experiment.

$1.0140 =$ diffusion-volume of carbonic oxide, by theory.

In the case of the last three gases, when the experiment was performed over water in a diffusion-tube, with free exposure to the dry atmosphere, instead of any contraction ensuing, a positive expansion generally occurred, which was to be attributed to the return air, which was comparatively dry, being expanded after entering the receiver.

14. *Carbureted Hydrogen of Marshes.*—Specific gravity, 0.555. Diffusion-volume, 1.3414.

In an experiment with this gas, deducting a small quantity of air which it contained, 252 measures were replaced by 187 air.

$\frac{252}{187} = 1.344 =$ diffusion-volume, by experiment.

$1341 =$ diffusion-volume, by theory.

These are all the permanent gases which could conveniently be submitted to diffusion. Vapours cannot be rigidly examined,

as they are all condensible in the pores of the stucco. The following Table exhibits a summary of the results:

TABLE

OF EQUIVALENT DIFFUSION-VOLUMES OF GASES; AIR = 1.

	By Experiment.	By Theory.	Specific Gravity.
Hydrogen,	3.83	3.7947	0.694
Carbureted Hydrogen,	1.344	1.3414	0.555
Olefiant Gas,	1.0191	1.0140	0.972
Carbonic Oxide,	1.0149	1.0140	0.972
Nitrogen,	1.0143	1.0140	0.972
Oxygen,	0.9487	0.9487	0.111
Sulphureted Hydrogen,	0.95	0.9204	1.1805
Protoxide of Nitrogen,	0.82	0.8091	1.527
Carbonic Acid,	0.812	0.8091	1.527
Sulphurous Acid,	0.68	0.6708	2.222

In the diffusion-volumes of oxygen, nitrogen, and carbonic oxide, the correspondence between theory and experiment is as close as could be desired. Indeed, admitting our law, I believe that the specific gravity of these gases can be determined by experiments on the principle of diffusion, with greater accuracy than by the ordinary means. But, to be of value, experiments performed with this important object in view, would require to be conducted with extreme care, in the most favourable circumstances, as regards uniformity of temperature, and to be frequently repeated. The diffusion-bulbs might also be considerably increased in size, and a greater minuteness of observation attained. Even in the most successful experiments recited in this paper, we cannot depend upon the absolute accuracy of the third decimal figure. In the case of carbonic acid gas, protoxide of nitrogen, sulphureted hydrogen, and sulphurous acid, the process of diffusion is interfered with in a greater or lesser degree by the absorbent action which all porous bodies exercise upon gases. Fortunately, however, the absorbent power of stucco is very low in degree.

The Density of any gas diffused into air, both being in the same state as to aqueous vapour, is obtained by the formula

$$D = \left(\frac{A}{G}\right)^2;$$

where G is the volume of gas submitted to diffusion, and A the volume of return-air. In operating upon gases lighter than air, the most useful instrument is a bulb of about two inches in diameter blown upon half-inch tube, of which about an inch may be left on either side of the bulb. The capacity of the instrument, *used as a gas-receiver over water*, is most simply determined by filling it with water, and weighing the water which it contains, and which can be poured from it into a counterpoised phial. Then, after any experiment, the return-air may be found from the weight of the water which has entered the instrument, determined in the same manner. By proceeding in this way, we avoid wetting the stucco after every experiment. A hood of damp paper may be inverted over the upper tube while the diffusion is going on, and the whole counterpoised in a tumbler of water, being suspended from one of the arms of the beam of a balance, the scale on that side being removed. An experiment with the bulb will generally occupy several hours. But with a plain diffusion-tube, a much shorter time will suffice.

A peculiar advantage of this mode of taking the specific gravity of gases, besides its simplicity, is, that we can operate upon a most minute quantity of gas: it is possible to come within 100th of the specific gravity, operating upon no more than one cubic inch of gas.

It is to be regretted that this method is not so fully available in the case of coal-gas, as might be expected. The density of that gaseous mixture appears to depend, in no inconsiderable measure, upon the presence of a small quantity of the heavier hydro-carburets, such as naphtha-vapour; and these are apt to be absorbed and withdrawn in part by the water, during the continuance of a diffusion experiment. I have observed coal-gas to con-

tract $\frac{1}{10}$th of its bulk by standing over water, without agitation, for forty-eight hours, and from the loss of the denser portion of it. But in the case of this gas, the experiment should succeed over brine, which absorbs much less of the gas than water does.

The process of diffusion may be managed so as to demonstrate relations in density. The short upper tubes of two diffusion-bulbs, not closed by plaster, but open, were connected by means of thick caoutchouc adopters, with the two ends of a short piece of straight tube, in which there was a diaphragm of plaster, $\frac{1}{3}$th of an inch in thickness, and equidistant from either end of the tube. The apparatus being proved air-tight, and the plug in a proper condition for diffusion, one of the diffusion-bulbs was filled with nitrogen gas, and the other with carbonic oxide, and the bulbs placed upright in separate contiguous glasses containing water. The quantity of gas in each was carefully observed at the beginning of the experiment, and after the expiry of twenty-four hours, when it was found to be identically the same as at first; at least, if a contraction or expansion took place, it was the same in both bulbs, and therefore entirely due to changes in temperature or pressure. Now, the gases were found by analysis to be uniformly diffused through both bulbs; so that nitrogen and carbonic oxide are of the same density, or at least do not differ more than $\frac{1}{500}$th part, which was the limit of observation in the case of these experiments. It appears, also, that inequality of density is not an essential requisite in diffusion.

I had occasion to remark, more than once, a singular accident to the stucco plugs. After being disused for some days or weeks, and left in the interval exposed to the air, which might be either dry or damp at the time, the plugs occasionally, on a new trial, did not permit diffusion to take place through their pores, at least immediately. Hydrogen, however, always opened a passage in the course of two or three minutes, and then the diffusion proceeded as rapidly as ever. Carbureted hydrogen, and the other gases, often required a longer period. A slight heat re-

stored the action of the plug. The obstruction could not be attributed to moisture, nor to any thing but dust.

It may be mentioned, that there was nothing peculiar in a mixture of two gases, in the proportion of the numbers expressing their diffusion-volumes;—nothing that could be considered an indication of mutual saturation.

Evaporation, or the elevation of vapour from a liquid into air, or any other gas, comes now to be explained on the principles of diffusion. The powerful disposition of the particles of different gaseous bodies to exchange positions, may as effectually induce the first separation of vapour from the surface of the liquid, as a vacuum would do. Once elevated, the vapour will be propagated to any distance, by exchanging positions with a train of particles of air, according to the law of diffusion. The length to which this diffusion proceeds, in a confined portion of air, is limited by a property of vapour, namely, that the particles of any vapour condense when they approximate within a certain distance. Hence, the quantity of vapour which rises into air, has the same limit as that which rises into a vacuum, and is the same.

I may be allowed to mention an application of the law of diffusion, in explanation of the mechanism of respiration. The cavity into which air enters during respiration, consists, first, of a large tube, the windpipe; secondly, of smaller tubes, into which the windpipe diverges; and, thirdly, of a series of still smaller tubes, diverging from the last, themselves ramifying to an indeterminate extent, till at last the tubes cease to be of sensible magnitude, but are believed to terminate in shut sacs. The capacity of the whole cavity cannot easily be determined, but we may estimate it at 300 cubic inches. In a natural expiration, about 20 cubic inches, or $\frac{1}{15}$th of the contents are thrown out, from the application of a general pressure to the whole. But it is evident, that these twenty cubic inches will be the twenty cubic inches nearest the outlet, or the contents of the larger tubes. The con-

tents of the second-sized tubes will advance at the same time into the largest tubes, but no further, and will recede again into their original depositories on the next inspiration, which will fill the larger tubes with fresh air; which identical quantity will again be expelled in the next expiration. This illustration is perhaps too strongly stated; but it is evident, that, in ordinary respiration, the slight mechanical compression will have little or no effect in emptying the most distant tubes, or the ultimate air-cells, of their contents. The bulk of the air, also, is not altered during respiration, although, for a quantity of oxygen, carbonic acid gas is substituted. This substitution, which is the great end of respiration, undoubtedly takes place most abundantly in the minute and distant air-cells, which present the largest surface to the blood; and the carbonic acid there produced, must be moved along the smaller tubes by the diffusion process, (which we know to be extremely energetic, and also inevitable), till it is thrown into the larger tubes, from which it can be expelled by the ordinary action of respiration. But the action of diffusion is always twofold: at the same time that carbonic acid is being carried outward from the air-cells, oxygen is carried inward in exchange, and thus the necessary circulation kept up throughout the whole lungs.

Farther, by a forced expiration, from 160 to 178 cubic inches may be expelled, after which, there still remain in the lungs about 120 cubic inches, which are not under the control of the respiratory action.

There can be no doubt that much of this quantity occupies constantly and permanently the most minute tubes and air-cells, for it can scarcely be withdrawn by means of the air-pump. Now, the question has arisen, how these ultimate tubes and air-cells are so powerfully inflated; for they are not distended by the action of muscular fibre, of which they are known to be destitute. This state of distention must be highly useful, by exposing surface; and the law of diffusion enables us to account for it.

The heavy carbonic acid which these minute cells may contain, is not merely exchanged for oxygen, but for a larger volume of oxygen, in the proportion of the diffusion-volumes of carbonic acid and oxygen, namely, 81 carbonic acid are replaced by 95 oxygen. The resistance to passage through the most minute tubes, is overcome by the diffusion action, as in the case of the pores of the stucco-plug, and there follows a tendency to accumulation on the side originally occupied by the carbonic acid. This accumulation is limited by the increased facility with which the air-vessels can empty themselves mechanically of a portion of their contents, from their distended state.

In the law of diffusion of gases, we have, therefore, a singular provision for the full and permanent inflation of the ultimate air-cells of the lungs.

But it is in the respiration of insects, that the operation of this law will be most distinctly perceived. The minute air-tubes accompanying the bloodvessels to every organ, and like them ramifying till they cease to be visible under the most powerful microscope, are kept distended during the most lively movements of the little animals, and the necessary gaseous circulation maintained, wholly, we may presume, by the agency of diffusion.

In regard to the terms of the law of diffusion: "The diffusion, or spontaneous intermixture of two gases in contact, is effected by an interchange in position of *indefinitely minute volumes of the gases*." My experiments, published on a former occasion, on the diffusion of mixed gases (*Quarterly Journal of Science*, Sept. 1829), afford the first demonstration of the fact, that diffusion takes place between the ultimate particles of gases, and not between sensible masses, and therefore that diffusion cannot be the result of accident. For, in the case of a mixture of two gases escaping from a receiver into the atmosphere, by apertures of 0.12 and 0.07 inch in diameter, it was not so much of the mixture which left the receiver in a given time, but a certain proportion of each of the mixed gases, independently of the other, cor-

responding to its individual diffusiveness. The same separation of mixed gases occurred in diffusion through the pores of stucco, or the fissure of a cracked jar.

" Which volumes are not necessarily of equal magnitude, being, in the case of each gas, inversely proportional to the square root of the density of that gas." This may be demonstrated, when different gases communicate by very narrow channels, or by very small apertures, and when inequality of pressure is guarded against. In the case of a gas communicating with the air by a wide aperture, on the other hand, although the diffusion or intermixture takes place precisely in the same way, still the result is different; for where a contraction takes place from the process of diffusion, the air flows in mechanically through the aperture, wholly unresisted, and makes up the deficiency. A gas, however, of large diffusion-volume escapes, in these circumstances, *in a shorter time* than a gas of small diffusion-volume. Indeed, it was the conclusion of the former paper, that gases diffuse more or less rapidly according to some function of their densities, " apparently inversely as the square root of their densities." The advantage, in illustrating the process of diffusion, of minute apertures or channels of communication, such as we have in the stucco-plug, depends upon the circumstance, that when a contraction or expansion takes place in the gaseous contents of a diffusion-instrument, any current in an outward or inward direction is prevented by frictional resistance; so that the simple result of diffusion is exhibited, not complicated by the effect of any other force.

The law at which we have arrived (which is merely a description of the appearances, and involves, I believe, nothing hypothetic), is certainly not provided for in the corpuscular philosophy of the day, and is altogether so extraordinary, that I may be excused for not speculating farther upon its cause, till its various bearings, and certain collateral subjects, be fully investigated.

On the Equations of Loci traced upon the Surface of the Sphere, as expressed by Spherical Co-ordinates. By Thomas Stephens Davies, Esq. F. R. S. Ed. F. R. A. S.

(Read 16th January 1832.*)*

The modern system of analytical geometry of three dimensions originated with Clairault, and received its final form from the hands of Monge. Descartes, it is true, had remarked, that the orthogonal projections of a curve anyhow situated in space, upon two given rectangular planes, determined the magnitude, species, and position of that curve; but this is, in fact, only an appropriation to scientific purposes of a principle which must have been employed from the earliest period of architectural delineation—the orthography and ichnography, or the ground-plan and section of the system of represented lines. Had Descartes, however, done more than make the suggestion—had he pointed out the particular aspect under which it could have been rendered available to geometrical research—had he furnished a suitable notation and methods of investigation—and, finally, had he given a few examples, calculated to render his analytical processes intelligible to other mathematicians;—then, indeed, this branch of science would have owed him deeper obligations than it can now be said to do. Still I do not wish to be understood to undervalue the labours of that extraordinary man, *indirectly* at least, upon this subject; for it is certain, that, though he did succeed in placing the inquiry in its true position, yet it is *ultimately* to his method of treating plane loci, by means of indeterminate algebraical equations, that we owe every thing we know in

the Geometry of Three Dimensions, beyond its simplest elements, as well as the most interesting and important portions of the doctrine of plane loci itself. I only wish to express, that the *direct* labours of Descartes on this subject are of very little value, if of any whatever; and this will be at once conceded, if we consult the Jesuit Rabuel's Commentary upon his *Geometria*, published nearly a century after the original work, and containing the most amplified attempt to illustrate this particular part of the treatise that had then been made. The writings, too, of Leibnitz and the two Bernoullis may be searched in vain for any attempts to lay down new methods, or to develope and apply that of Descartes for the general discussion of curve surfaces, or of curves any how situated in space. Indeed, we cannot but be struck with the very small number of investigations of this kind attempted by them; and even those few might be said to have arisen out of the rivalry between the old geometry and the new. The singular facility possessed by these illustrious men, of devising methods of inquiry—of modifying those already in existence, to accomplish the purpose they had in view—and their devotion to every thing that could set the powers of the calculus in a more advantageous light,—these would certainly have led them to the consideration of the subject, had the method of Descartes been adequately stated, and its adaptation to any kind of calculus been even tolerably obvious. James Bernoulli did, indeed, throw out a casual observation in the Leipsic Acts, that *an equation between three variables represented a curve surface, as an equation between two variables represented a plane curve;* but the remark seems to have suggested no further inquiries, and so far as James Bernouilli was concerned, the subject dropped at this point. Clairault, by an independent train of inquiry, and whilst yet a minor, was conducted to the same result; but, pushing the investigation one step further, he was led to consider *every curve line in space as the intersection of two curve surfaces,* or as a line

traced upon them both at the same time. Every thing else followed, and it soon became the universal practice to investigate the properties of curves of double curvature by reference to three co-ordinate planes, and determined by any two equations between the three variables. The method of DESCARTES at best led only to the discussion of curves of double curvature, by means of the equations $f_{,}zy$ and $f_{,,}zx$, instead of $f'xyz$ and $f''xyz$, each equated to zero; whilst the latter process, which is that of CLAIRAULT, embraced the former as a particular case, granting, however, the resolution of equations of all orders: but that of DESCARTES could in fact only be applied in this particular case, whilst that of CLAIRAULT is independent of any such necessity, and applies with the same success, whether the elimination of any variable between the given equations be possible or not.

The problem of VIVIANI, which was professedly proposed to bring the powers of the new Geometry to the test, must have awakened attention to this class of inquiries; and with respect to spherical loci, several problems of considerable difficulty were attempted during the next forty years, with varied degrees of success. The methods almost invariably employed in these inquiries was, to consider the character of the projection of the curve upon the equatorial plane, and, by means of these and the relations between the current vertical ordinates of the sphere, to express all the conditions, relations, and results. A few exceptions, perhaps, might be made to this statement, the only one of which I can call to mind is that of JAMES BERNOULLI's solution of VIVIANI's Florentine Paradox, where he takes only account of arcs of the meridian of the current point, and the angles which those meridians form with a certain given meridian. He does not, however, seem to entertain the slightest notion of its applicability to any other class of problems than that one before him. When CLAIRAULT's principle, however, became known, all those particular artifices were merged in that general method; and the sphe-

rical curves, like all others, were expressed by two equations between three variables, one of which equations was that of the sphere itself. From that time forward, we find no traces of any attempt to devise *special methods* adapted to the discussion of curves traced upon specific surfaces, and to them only, amongst the continental mathematicians. No doubt, so far as the investigation of properties common to all classes and all orders of curves was concerned, this was an improvement of incalculable value, and one which for that purpose was altogether indispensable; but it became at the same time a barrier to the inquiries respecting the peculiar characters of that class of curves which are traced on a spherical surface. The sphere, indeed, from the uniform character of all the regions about any point of its surface, would, we should at first sight expect, possess a great variety of interesting, and possibly, of important, properties belonging to itself alone, in the same manner as the circle does amongst plane curves; and certainly, by methods whose essential character is their adaptation to the discovery of affections common to all curves, or to all curve-surfaces, we can scarcely hope to discover those which are *peculiar* to one class amongst them. The importance of the sphere in astronomical inquiries (especially in that approximative astronomy which considers the earth a sphere, and its orbit a circle), as well as to the discussion of several curious points connected with astronomical history,—these considerations render it an object of great importance that we should be able to investigate its loci by means of a system adapted to that purpose only, and therefore divested of every thing foreign to that immediate object. Some *direct* and simple methods of investigation are essential to our intelligibly expressing the varieties of the celestial motions, in a manner adapted to subsequent determination of the loci which result from their combinations. One instance of the advantages of such a method has been given in my paper on the "Antique Hour-Lines," which the Royal

Society of Edinburgh has done me the honour to insert in its Transactions; and at a future time I hope to confer the same simplicity and completeness upon some other questions which have been repeated subjects of unsatisfactory discussion by every other process. My present purpose, however, is to lay down a sketch of the systematic principles of the method, to furnish solutions to the problems that naturally present themselves at the outset of the inquiry, and to apply it to the discussion of several problems, that, by their repeated agitation, have acquired a celebrity which gives them a high mathematical interest, independent of any practical utility they might be supposed to possess in the affairs of common life.

The method which I propose to employ is an equation between two spherical variables. Sometimes the latitude and longitude, sometimes the polar distance and the polar angle, will be the more convenient. These two cases have a much closer analogy on the sphere than the methods by polar and rectangular co-ordinates have *in plano*. The former is that generally employed in the hour-lines; the latter will be employed almost exclusively here, the class of problems here discussed seeming generally to admit of more ready treatment under this than the other form. The methods are, however, readily convertible, so that we can pass and repass from one to the other without that difficulty and complexity which commonly attends such transitions in the case of rectilinear co-ordinates. All this is very simple, and will probably have often occurred to mathematicians before: yet there is still only one single exception to the statement, that no direct and promising attempt has ever been made to put it in practice *, and even that was dropped in the very outset. The great difficulty amongst the earlier geometers was found to be the inconvenient notation by which trigonometrical relations were im-

* See Note E.

pressed. The arc or angle never appeared in the notation before the time of EULER: all the functions of it were expressed as functions of some one selected function of the arc, rather than directly as functions of the arc itself. Symbols of operation, though the most natural symbols, and perhaps more easily apprehended in the infancy of mathematical study, were then used very sparingly, and were scarcely ever themselves rendered subject to subsequent operations; except, indeed, when they assumed a numerical form in a given position, as, for instance, the exponent of a power. It was also undecided then, and is scarcely agreed upon even now, in what way angles become subject to calculation in conjunction with linear quantities. The result of trigonometrical operations, thus shackled, became so extremely complex, and the transformation which they required so difficult, that any geometer who attempted it quickly abandoned it in utter hopelessness. The trigonometrical notation of EULER, by attaching the name of the line as a functional characteristic to the symbol of the arc, opened a new and fertile field for discovery, and trigonometry instantly assumed a renovated aspect under the talisman of his magic touch. Had there been then any disposition to pursue the investigation of spherical loci, the chief difficulty was removed; and, no doubt, it would soon have assumed, in his hands, the simplicity and symmetry which MONGE was at the same time labouring to confer upon the geometry of rectilinear co-ordinates. There was, indeed, still wanting the notation for inverse trigonometrical functions, which has been more recently and elegantly supplied by a living mathematician, whose varied intellectual powers approach more closely to those of EULER than any one who has yet appeared since the world was deprived of that singularly gifted mind. The want of this notation was not so strongly felt; but it would have been sooner felt, and sooner discovered, had the cultivation of spherical loci, by means of trigonometrical functions, been earlier cul-

tivated. But the more general form which loci, situated in space, had assumed, caused this to be looked upon as an unimportant, or at least so subordinate, a method, as to be unworthy of cultivation; and it was therefore taken up by no one competent to complete the task which Euler had left ready to be entered on. The beautiful theorem of Lexell, however, which assigns the locus of the vertical angle whose base and area are given, would, we should have been led to expect, have recalled attention to the subject: but it did not;—and even to the present day, amongst the continental geometers, no traces of a system of spherical co-ordinates are to be found. It is the more singular, inasmuch as several spherical loci, which are known by other methods to be *circles*, have already been determined to be so by trigonometrical considerations. *Yet none of these methods are capable of showing* à priori *what the locus is; they only show whether the suspicion previously entertained (for whatever reason) of their being circles, be or be not true.* We may refer to the demonstrations given by Lagrange and Legendre to Lexell's theorem, as specimens, these being amongst the most elegant of this class. The method of spherical co-ordinates has been for some years familiar to my mind, and I have occasionally used it in the occasional inquiries which presented an opportunity of applying it, and generally with complete success: and, indeed, it seemed so natural and obvious a method of proceeding, that I could not, till after considerable search and frequent consultation of my mathematical friends, persuade myself that it had not been already discussed, and its principles and application properly detailed. The near approach to it in several of the discussions concerning the rhumb-line (especially that of Dr Hutton), and more particularly the method of expressing the angle which it made with the meridian, rendered me so doubtful on this point, that I did not venture to state, in my paper on the Hour-Lines, that the *general method* of investigating spherical loci, of which the Hectimoria furnished

a specimen, had any thing of originality in it. Subsequent inquiries have convinced me that it has not been developed. In the latter part of the summer, when reading the proofs of my paper (which recalled my attention to the subject), I resolved forthwith to undertake a complete analysis of its first principles, and ascertain whether there were any *insuperable difficulties in the details,* which deterred former mathematicians from the pursuit. I did so, and can now see pretty clearly the source of its general neglect. *The equation of a curve, when obtained, was not commonly capable of ready and obvious interpretation, any symmetry of which it was capable rarely extending to a symmetrical function of both variables; those equations did not admit of classification in any way analogous to those of rectilinear co-ordinates; the processes themselves were of a class very different from those with which the majority of mathematicians were most familiar; and the tendencies of the transformations, often of considerable extent, exceedingly difficult to foresee.* Few, therefore, who had not had the good fortune to be successful in some interesting collateral investigations, would have had the resolution to proceed with an attempt so apparently endless and hopeless as to systematise the mass of results that flowed from these preliminary, and in some degree conjectural, inquiries. This last advantage I did possess, and I hope the results given in the present dissertation will show that it affected me properly. The road is now fairly opened, and I lose no time in pointing it out to mathematicians having leisure and inclination for such pursuits. As a whole, however, this paper may be said to be incomplete; but that has arisen from want of room, as in all cases the results admit of the same degree of completeness as that given to the problems actually discussed. I have, however, dwelt rather largely upon the equations of the circle, both because it stands the most prominent, and perhaps most interesting,

of all spherical loci. It enters into discussions respecting these in the same way as the straight line does into the geometry of rectilinear co-ordinates; that is, as its generatrix, and as the simplest line that can be traced upon the surface: and hence, whatever might be the class of problems that should form the principal subject of our investigation, it appeared certain that no general system could be devised, which did not include the equation of the circle as one of its most important elements. That this may be the case, it became necessary to assign the equations of circles, great and less, subject to every variety of defining conditions. The expressions here given, are, it is true, sometimes long, but they always involve symmetrical data symmetrically in the result: and on that account are preferable, both for their generality of application, and their greater elegance as analytical formulæ. Those cases where greatest simplicity is required are obtained at once, by giving suitable values to the data of the problem; or they may be so transformed by taking suitable origin and direction of the co-ordinate axes. There is, besides, but little difficulty in discovering how this is to be done; and I have not, therefore, dwelt upon it in treating of the circle. In one or two cases where the discovery of a truth was the object in view, rather than the discovery of a general formula, I have not scrupled to adopt this shorter process; and in others, the inquiry would have given results so complex as to be incapable of reduction, had I not taken that method of simplification. This, however, is only identical with our common practice in the geometry of rectilinear co-ordinates, and if we urge it as a difficulty against one method, it applies with great force against the other. I have no doubt, then, that when we have become as familiar with this mode of inquiry as with the other, we shall find it equally simple, easy, and intelligible, as that is to us now.

In the next place, I have given formulæ of transformation of

spherical co-ordinates, both as to origin and direction. Then follow *formulæ of projection*, for ascertaining the equation of the projection of any spherical locus, upon a plane parallel to the tangent plane at the origin of spherical co-ordinates. They are both simple and symmetrical; yet simple as they are, they are capable of universal application. Amongst these projections, I particularly notice the three usual ones,—the orthographic, gnomonic, and stereographic, which are the only cases possessing any analytical peculiarity.

The remainder of the paper is devoted to the consideration of various spherical curves; and the results are often curious and unexpected. I have, however, in many cases, from studying the brevity necessary in a dissertation of this kind, been obliged to omit discussions which would have repaid our attention to them, had our limits permitted us to dilate.

The first curve is the *equable spherical spiral*, a family which contains several such loci as leave a quadrable residue on the surface of the sphere. Amongst them are the spiral of Pappus, the oval window of Viviani, and some others worthy of notice. The connexion between these curves has never before been noticed; nor has it before been shown that the construction given by James Bernoulli for solving Viviani's problem is the same as that of the proposer himself. In the next place follows the investigation of spherical loci, produced in the same way as the conic sections are *in plano* referred to two foci, and which I hence call the spherical-ellipse, hyperbola, and parabola. It is shown that the ellipse and hyperbola so generated (that is, the locus of the point, the sum of whose distances, and that the difference of whose distances, are constant), may, as *in plano*, be comprised in one single equation: that the loci themselves, in each case, formed a double system of lines, were in all respects equal and similar, but surrounding the two poles of the sphere, so that they would have been pro-

duced by the perforation of the sphere by a particular cylinder. The curve upon the earth to which the sun is vertical during his annual path has also been assigned, and its identity with the most general form of the equable spiral has been pointed out. I have given also an investigation of LEXELL's theorem, and another similar locus, well known to geometers, the envelope of the base of a triangle, the sum of whose sides and whose vertical are given. They are solved, I believe for the first time, in a perfectly legitimate manner. That is to say, in such a way as would have led to the discovery of the locus itself, without any previous suspicion being entertained as to what the locus was,—by finding the equations of those loci, and shewing they were the previously determined equations of the circle. It would obviously have been easy to extend this class of processes to all cases which have been considered; and I trust that there will be sufficient attention given to the subject by mathematicians to supply a numerous assemblage of elegant problems, for the use of the student just entering upon the inquiry,—a purpose for which they are well adapted.

The spherical epicycloid, a curve which had been considered by HERMANN, JOHN BERNOULLI, MAUPERTIUS, NICOLE, and CLAIRAULT, is also brought under examination by this method, and investigated on the most general supposition, viz. when the tracing point is not in the circumference of the rolling circle. I examine the expression for its length in reference to the theorem of JOHN BERNOULLI, that if *the rolling circle be a great circle, and the tracing point in its circumference, the curve traced out will be rectifiable, and I skew that it is rectifiable in no other case.*

In discussing the loxodrome, I have given the three projections of the curve on the equator, and examined the course of the curve itself on the surface of the sphere. As I had confined my plan, in the present paper, to a discussion only of such properties of spherical curves as were deducible from the expres-

sions of their equations, without the aid of expansions and differential coefficients, I am compelled to omit many properties of this curve which are exceedingly curious,—not less curious than those which belong to its stereographic projection, the logarithmic spiral, which induced JAMES BERNOULLI to designate it by the name *spiral mirabilis.* Finally, I slightly examine a locus which has considerable physical interest, especially arising out of the elegant experiment of Mr BARLOW, to shew "the probable electric origin of the phenomena of terrestrial magnetism," detailed in the last fasciculus of the Philosophical Transactions.

In pursuance of a plan like the present, it was impossible to concentrate every thing I had to say in the text: and therefore, I insert a few notes and illustrations, in the form of addenda to the paper itself. It only remains for me to apologise for the length of this explanatory and historical preface, and to express my regret that it could not be compressed into smaller space, without ceasing to answer the purpose for which it was intended.

BATH, *November* 21. 1831.

DEFINITIONS.

THE circle may be considered either as a curve traced upon a sphere or upon a plane. When we consider it to be traced upon a plane, its centre and radius are also in that plane, and may, for distinction, be called the *plane-centre* and the *plane-radius*, respectively, of that circle. But when it is considered as a curve traced upon the sphere, we may call the centre and radius (what are usually called the pole and polar distance of the circle) by the corresponding name of *spherical centre and spherical radius* of the circle.

In the investigations which follow, we rarely have occasion to speak of the plane-centre and plane-radius, so that no confusion can arise from dropping the adjective spherical, when speaking of the spherical centre and spherical radius. When, however, we apprehend that any misunderstanding might arise from the omission, we shall be careful to supply the adjectives.

When we consider any curve traced on the surface of the sphere, we consider it, as in other parts of geometrical inquiry, to be the locus of a point, which fulfils certain given conditions, and that the algebraical expression of these conditions may be reduced to an equation between two variable arcs of great circles, or between an arc and an angle. When between two arcs, they are supposed at right angles, like the prime meridian and equator, in describing the latitude and longitude of a place on the globe. The longitude of the point we call the abscissa, and its latitude the ordinate. When we refer to the polar angle made by the meridian of the point with the prime meridian, and the polar distance of this point, we call it a polar equation, the polar distance being the *radius-vector*, and the *polar angle* is the longitude of the point.

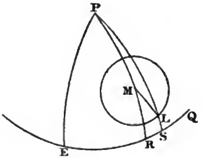
These two methods can only just be said to differ, since in one, the polar angle is measured by the abscissa of the other, and the radius-vector in one is the complement of the ordinate of the other. In principle, then, they are strictly the same, and in the details nearly so: though it will often happen that one consideration is more directly applicable to particular problems than

the other, on account of a closer alliance to the form in which the data is exhibited. Whether it may be generally so or not, future experience alone can decide; but my own observations lead me to think, that the polar method is capable of greater facility of application to spherical loci than the geographical. But, as the radius-vector and polar angle are so related to the latitude and longitude, we may at any step change from one to the other consideration, without the slightest mental effort, and without any analytical reduction whatever.

I.

THE EQUATIONS OF THE CIRCLE ON THE SPHERE.

To find the equation of a circle whose centre and radius on the surface of a sphere are given.



Let M be the centre, P the pole, EQ the equator, PE the first meridian, and L any point in the circumference, whose equation is sought. Denote by λ and κ, the radius-vector PM and polar angle EPM of the centre M, and by ρ the radius ML of the circle. Let EPL $=\theta$, and PL $=\phi$. Then we have at once

$$\cos\rho = \cos\lambda\cos\phi + \sin\lambda\sin\phi\cos(\theta-\kappa), \qquad \text{(1.)}^*$$

which is the equation required in its most general form.

* See, for other forms, Note A, at the end of this paper.

Cor. 1. Let $\rho = \frac{\pi}{2}$; that is, the circle be a *great circle;* then its equation becomes

$$\cot \phi = -\tan \lambda \cos (\theta - \kappa), \dots\dots (2.)$$

$$\text{or } \cos (\theta - \kappa) = -\cot \lambda \cot \phi, \dots\dots (3.)$$

which are the equations of a great circle whose centre has the position λ, κ; according as it is resolved for one or other of the variables.

Cor. 2. Let $\lambda = 0$; that is, let the centre of the circle (1.) be at the pole of the equator, then the circle is

$$\cos \rho = \cos \phi, \dots\dots (4.)$$

which is the equation of a parallel of latitude, or declination, according as the terrestrial or celestial sphere is used.

Cor. 3. At the same time, let $\lambda = 0$, and $\rho = \frac{\pi}{2}$; then the equation becomes

$$\cos \phi = 0, \text{ or } \phi = \frac{\pi}{2}, \dots\dots (5.)$$

which is the equation of the equator itself.

Cor. 4. Let $\lambda = \frac{\pi}{2}$; then equation (1.) becomes

$$\cos \rho = \sin \phi \cos (\theta - \kappa), \dots\dots (6.)$$

the equation of a less circle, whose pole is in the equator.

Cor. 5. Let also $\rho = \frac{\pi}{2}$; then

$$\sin \phi \cos (\theta - \kappa) = 0 \dots\dots (7.)$$

This is fulfilled both by $\sin \phi = 0$, and $\cos (\theta - \kappa) = 0$. The interpretation is, that whilst $\sin \phi = 0$, we have $\cos (\theta - \kappa)$ indeterminate, and whilst $\cos (\theta - \kappa) = 0$, then $\sin \phi$ is indeterminate. The former of these shews, that whatever value be given to θ, the circle will pass through $\sin \phi = 0$, or P; and the latter, that whatever value be given to $\sin \phi$, the equation of longitude is always the same. The latter is properly the equation of any one definite meridian, the longitude of whose centre is κ; the former shews,

whatever point in the equation be taken as the centre of a great circle, that great circle will pass through the pole of the equator *.

Cor. 6. The intersection of the equator with the circle (1.) will be found by combining it with $\lambda = \frac{\pi}{2}$. This gives

$$\cos(\theta - \kappa) = \frac{\cos\rho}{\sin\lambda}, \quad \ldots\ldots (8.)$$

Cor. 7. If $\sin\lambda < \cos\rho$, then $\cos(\theta - \kappa) > 1$, which indicates impossibility.

Cor. 8. Let, also, $\rho = \frac{\pi}{2}$; then

$$\cos(\theta - \kappa) = 0, \text{ or } \theta = \frac{\pi}{2} + \kappa. \quad \ldots\ldots (9.)$$

the longitude of the point of intersection.

Cor. 9. All great circles whose centres are in the same meridian, intersect the equator in the same points; for equation (9.) is independent of λ, and shews that the point is at the distance of a quadrant from the longitude of the centres in question. It thus agrees with a simple property which enters into the elements of the old spherical geometry.

II.

To find the distance between two given points on the surface of the sphere.

Let the co-ordinates of the points be $\alpha_{,}\,\beta_{,}$ and $\alpha_{,,}\,\beta_{,,}$; then, if δ = distance, we have

$$\cos\delta = \cos\alpha_{,}\cos\alpha_{,,} + \sin\alpha_{,}\sin\alpha_{,,}\cos(\beta_{,,} - \beta_{,}).$$

---

* It is altogether unnecessary to prove, that, for $\frac{\pi}{2}$ in these and most of the other results of this paper, we may substitute $(2n+1)\frac{\pi}{2}$, where n is any whole number positive or negative.

III.

We may from this and a known theorem in spherics, *express the area of a spherical triangle, in terms of the co-ordinates of the three angles of that triangle.*

For, let $\alpha_{,}\beta_{,}$, $\alpha_{,,}\beta_{,,}$, and $\alpha_{,,,}\beta_{,,,}$ be the three pairs of co-ordinates, then we have as above,

$$\left.\begin{aligned}\cos\delta_{,} &= \cos\alpha_{,,}\cos\alpha_{,,,} + \sin\alpha_{,,}\sin\alpha_{,,,}\cos\overline{\beta_{,,}-\beta_{,,,}}\\ \cos\delta_{,,} &= \cos\alpha_{,}\cos\alpha_{,,,} + \sin\alpha_{,}\sin\alpha_{,,,}\cos\overline{\beta_{,}-\beta_{,,,}}\\ \cos\delta_{,,,} &= \cos\alpha_{,}\cos\alpha_{,,} + \sin\alpha_{,}\sin\alpha_{,,}\cos\overline{\beta_{,}-\beta_{,,}}\end{aligned}\right\}\text{..............(1.)}$$

But, combining the expressions of Du Gua and Cagnoli for the spherical excess, we have

$$\cos\frac{\epsilon}{2} = \frac{1+\cos\delta_{,}+\cos\delta_{,,}+\cos\delta_{,,,}}{\sqrt{2(1+\cos\delta_{,})(1+\cos\delta_{,,})(1+\cos\delta_{,,,})}}, \text{ ....(2.)}$$

which gives, by means of (1.), the following symmetrical formula for the spherical excess, in terms of the co-ordinates of the angles of the triangle,

$$\cos\frac{\epsilon}{2} = \frac{1+\cos\alpha_{,}\cos\alpha_{,,}+\cos\alpha_{,}\cos\alpha_{,,,}+\cos\alpha_{,,}\cos\alpha_{,,,}+\sin\alpha_{,}\sin\alpha_{,,}\cos\overline{\beta_{,}-\beta_{,,}}+\sin\alpha_{,}\sin\alpha_{,,,}\cos\overline{\beta_{,}-\beta_{,,,}}+\sin\alpha_{,,}\sin\alpha_{,,,}\cos\overline{\beta_{,,}-\beta_{,,,}}}{\sqrt{2(1+\cos\alpha_{,}\cos\alpha_{,,}+\sin\alpha_{,}\sin\alpha_{,,}\cos\overline{\beta_{,}-\beta_{,,}})(1+\cos\alpha_{,}\cos\alpha_{,,,}+\sin\alpha_{,}\sin\alpha_{,,,}\cos\overline{\beta_{,}-\beta_{,,,}})(1+\cos\alpha_{,,}\cos\alpha_{,,,}+\sin\alpha_{,,}\sin\alpha_{,,,}\cos\overline{\beta_{,,}-\beta_{,,,}})}}\text{(3.)}$$

By means of a series of subsidiary theorems, I have been able to give this a form adapted to logarithms; but as the purpose of this paper has no reference to facility of calculation, and as, moreover, the process is long, and connected with inquiries which I may bring separately under the consideration of the Royal Society, it will be unnecessary to enter upon it here. In its present form, it will not, I think, be without interest to mathematicians.

IV.

To find the equation of a great circle passing through two given points on the sphere.

Let κ, λ be the unknown co-ordinates of the centre of the circle whose equation we seek, and let $\alpha_{,}\beta_{,}$ and $\alpha_{,,}\beta_{,,}$ be the co-ordinates of the two given

points. Then the equation of the circle is of the form

$$\cot\phi = -\tan\lambda\cos(\theta-\kappa), \qquad\qquad (1.)$$

And because $\alpha_{,}\,\beta_{,}$ and $\alpha_{,,}\,\beta_{,,}$ are points in its course, we have also

$$\cot\alpha_{,} = -\tan\lambda\cos(\beta_{,}-\kappa), \qquad\qquad (2.)$$

$$\cot\alpha_{,,} = -\tan\lambda\cos(\beta_{,,}-\kappa), \qquad\qquad (3.)$$

Divide (2.) by (3.), expand the cosines, and divide numerator and denominator of the right-hand number by $\cos\kappa$, which gives

$$\frac{\cot\alpha_{,}}{\cot\alpha_{,,}} = \frac{\cos\beta_{,}+\sin\beta_{,}\tan\kappa}{\cos\beta_{,,}+\sin\beta_{,,}\tan\kappa}, \qquad\qquad (4.)$$

Or, resolving (4.) with respect to $\tan\kappa$, we have

$$\tan\kappa = -\frac{\cot\alpha_{,,}\cos\beta_{,}-\cot\alpha_{,}\cos\beta_{,,}}{\cot\alpha_{,,}\sin\beta_{,}-\cot\alpha_{,}\sin\beta_{,,}}, \qquad\qquad (5.)$$

$$\sin\kappa = \mp\frac{\cot\alpha_{,,}\cos\beta_{,}-\cot\alpha_{,}\cos\beta_{,,}}{\sqrt{\cot^2\alpha_{,,}-2\cot\alpha_{,,}\cot\alpha_{,}\cos(\beta_{,,}-\beta_{,})+\cot^2\alpha_{,}}} \qquad\qquad (6.)$$

$$\cos\kappa = \pm\frac{\cot\alpha_{,,}\sin\beta_{,}-\cot\alpha_{,}\sin\beta_{,,}}{\sqrt{\cot^2\alpha_{,,}-2\cot\alpha_{,,}\cot\alpha_{,}\cos(\beta_{,,}-\beta_{,})+\cot^2\alpha_{,}}} \qquad\qquad (7.)$$

either of which gives the longitude, or polar angle, of the centre of the circle.

Again, from equations (2.) and (3.), we obtain

$$\cos^{-1}(-\cot\alpha_{,,}\cot\lambda)-\cos^{-1}(-\cot\alpha_{,}\cot\lambda)=\beta_{,,}-\beta_{,};$$

or, taking cosine function of each side, and transposing, we obtain

$$\{(1-\cot^2\alpha_{,,}\cot^2\lambda)(1-\cot^2\alpha_{,}\cot^2\lambda)\}^{\frac{1}{2}}=\cos(\beta_{,,}-\beta_{,})-\cot\alpha_{,}\cot\alpha_{,,}\cot^2\lambda;$$

or, squaring and performing obvious reductions,

$$\cot\lambda = \pm\frac{\sin(\beta_{,,}-\beta_{,})}{\sqrt{\cot^2\alpha_{,,}-2\cot\alpha_{,,}\cot\alpha_{,}\cos(\beta_{,,}-\beta_{,})+\cot^2\alpha_{,}}} \qquad\qquad (8.)$$

Inserting in (1.) the values of $\cot\lambda$, $\sin\kappa$, $\cos\kappa$, given by (6, 7, 8.) we find ultimately the equation of the circle sought

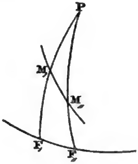
$$\cot\phi = -\operatorname{cosec}(\beta_{,,}-\beta_{,})\{\cos\theta(\cot\alpha_{,,}\sin\beta_{,}-\cot\alpha_{,}\sin\beta_{,,})\mp\sin\theta(\cot\alpha_{,,}\cos\beta_{,}-\cot\alpha_{,}\cos\beta_{,,})\} \qquad (9.$$

This may also be put under the form, and often advantageously,

$$\cot\phi = \frac{\cot\alpha_{,}\sin(\beta_{,,}\mp\theta)-\cot\alpha_{,,}\sin(\beta_{,}\mp\theta)}{\sin(\beta_{,,}-\beta_{,})}. \qquad\qquad (10.)$$

V.

To find the equation of a great circle on the sphere, which shall cut two given meridians at given angles.



Let $PF_{,}$ and $PF_{,,}$ be the meridians; $M_{,}$ and $M_{,,}$ the points where the great circle sought shall cut them; $\pi - \epsilon_{,}$ and $\epsilon_{,,}$ the two angles of the triangle formed by that circle and the given meridians and $\beta_{,,} - \beta_{,}$ the third. If we put, for the moment, $\pi - \epsilon_{,} = A$, $\epsilon_{,,} = B$, $\beta_{,,} - \beta_{,} = C$, and $A + B + C = 2\,S$, we have

$$\left.\begin{aligned}
S - A &= \frac{-\overline{\pi-\epsilon_{,}} + \epsilon_{,,} + \overline{\beta_{,,}-\beta_{,}}}{2} = \tfrac{1}{2}\{-\overline{\pi-\epsilon_{,}} + (\epsilon_{,,} + \overline{\beta_{,,}-\beta_{,}})\} \\
S - B &= \frac{\overline{\pi-\epsilon_{,}} - \epsilon_{,,} + \overline{\beta_{,,}-\beta_{,}}}{2} = \tfrac{1}{2}\{\overline{\pi-\epsilon_{,}} - (\epsilon_{,,} - \overline{\beta_{,,}-\beta_{,}})\} \\
S - C &= \frac{\overline{\pi-\epsilon_{,}} + \epsilon_{,,} - \overline{\beta_{,,}-\beta_{,}}}{2} = \tfrac{1}{2}\{\overline{\pi-\epsilon_{,}} + (\epsilon_{,,} - \overline{\beta_{,,}-\beta_{,}})\} \\
S &= \frac{\overline{\pi-\epsilon_{,}} + \epsilon_{,,} + \overline{\beta_{,,}-\beta_{,}}}{2} = \tfrac{1}{2}\{\overline{\pi-\epsilon_{,}} + (\epsilon_{,,} + \overline{\beta_{,,}-\beta_{,}})\}
\end{aligned}\right\}\quad\ldots\ldots\ldots(1.)$$

$$\left.\begin{aligned}
\text{Also, } \cot \alpha_{,,} &= \frac{\cot \tfrac{1}{2}\alpha_{,,} - \tan \tfrac{1}{2}\alpha_{,,}}{2} \\
\cot \alpha_{,} &= \frac{\cot \tfrac{1}{2}\alpha_{,} - \tan \tfrac{1}{2}\alpha_{,}}{2}
\end{aligned}\right\}\quad\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(2.)$$

But $\cot \frac{1}{2} a_{,,} = \left\{ \dfrac{\cos \overline{S-B} \cos \overline{S-C}}{-\cos S \cos \overline{S-A}} \right\}^{\frac{1}{2}} \therefore \tan \frac{1}{2} a_{,,} = \left\{ \dfrac{-\cos S \cos \overline{S-A}}{\cos \overline{S-B} \cos \overline{S-C}} \right\}^{\frac{1}{2}}$ . . (3)

Inserting these values in the first of equations (2), and performing a similar process with respect to $\cot a_{,}$, we shall obtain

$$\left.\begin{aligned} \cot a_{,,} &= \frac{\cos S \cos \overline{S-A} + \cos \overline{S-B} \cos \overline{S-C}}{2\sqrt{-\cos S \cos \overline{S-A} \cos \overline{S-B} \cos \overline{S-C}}} \\ \cot a_{,} &= \frac{\cos S \cos \overline{S-B} + \cos \overline{S-A} \cos \overline{S-C}}{2\sqrt{-\cos S \cos \overline{S-A} \cos \overline{S-B} \cos \overline{S-C}}} \end{aligned}\right\} \ldots\ldots\ldots\ldots (4.)$$

In the numerators of these expressions, put for A, B, C, S their values, and put 2 R instead of the denominator. Then we shall get

$$\cos \overline{S-B} \cos \overline{S-C} = \cos \tfrac{1}{2} \{\overline{\pi - \epsilon_{,}} - (\epsilon_{,,} - \overline{\beta_{,,} - \beta_{,}})\} \cos \tfrac{1}{2} \{\overline{\pi - \epsilon_{,}} + (\epsilon_{,,} - \overline{\beta_{,,} - \beta_{,}})\}$$

$$= \tfrac{1}{2} \{\cos \overline{\pi - \epsilon_{,}} + \cos (\epsilon_{,,} - \overline{\beta_{,,} - \beta_{,}})\}$$

$$\cos S \cos \overline{S-A} = \cos \tfrac{1}{2} \{\overline{\pi - \epsilon_{,}} + (\epsilon_{,,} + \overline{\beta_{,,} - \beta_{,}})\} \cos \tfrac{1}{2} \{-\overline{\pi - \epsilon_{,}} + (\epsilon_{,,} + \overline{\beta_{,,} - \beta_{,}})\}$$

$$= \tfrac{1}{2} \{\cos \overline{\pi - \epsilon_{,}} + \cos (\epsilon_{,,}' + \overline{\beta_{,,} - \beta_{,}})\};$$

These give $\cot a_{,,} = \dfrac{\frac{1}{2} \cos \overline{\pi - \epsilon_{,}} + \frac{1}{2} \cos (\epsilon_{,,} - \overline{\beta_{,,} - \beta_{,}}) + \frac{1}{2} \cos \overline{\pi - \epsilon_{,}} + \frac{1}{2} \cos (\epsilon_{,,} + \overline{\beta_{,,} - \beta_{,}})}{2 R}$

$$= \frac{\cos \overline{\pi - \epsilon_{,}} + \frac{1}{2} \{\cos (\epsilon_{,,} - \overline{\beta_{,,} - \beta_{,}}) + \cos (\epsilon_{,,} + \overline{\beta_{,,} - \beta_{,}})\}}{2 R}$$

$$= \frac{\cos \overline{\pi - \epsilon_{,}} + \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}}}{2 R}$$

$$= \frac{-\cos \epsilon_{,} + \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}}}{2 R} \ldots\ldots\ldots\ldots (5.)$$

$$\text{Similarly } \cot a_{,} = \frac{\cos \epsilon_{,,} - \cos \epsilon_{,} \cos \overline{\beta_{,,} - \beta_{,}}}{2 R} \ldots\ldots\ldots\ldots (6.)$$

For $\cot a_{,}$ and $\cot a_{,,}$, put their values just found in the equations (5, 6, 7, 8,) of Art. IV. Then we shall get

$$\tan \kappa = -\frac{(\cos \epsilon_{,} - \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}}) \cos \beta_{,} + (\cos \epsilon_{,,} - \cos \epsilon_{,} \cos \overline{\beta_{,,} - \beta_{,}}) \cos \beta_{,,}}{(\cos \epsilon_{,} - \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}}) \sin \beta_{,} + (\cos \epsilon_{,,} - \cos \epsilon_{,} \cos \overline{\beta_{,,} - \beta_{,}}) \sin \beta_{,,}} \ldots (7.)$$

$$\sin \kappa = \mp \frac{(\cos \epsilon_{,} - \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}}) \cos \beta_{,} + (\cos \epsilon_{,,} - \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta'}) \cos \beta_{,,}}{\sin \overline{\beta_{,,} - \beta} \sqrt{\cos^2 \epsilon_{,} - 2 \cos \epsilon_{,} \cos \epsilon_{,,} \cos \overline{\beta_{,,} - \beta_{,}} + \cos^2 \epsilon_{,,}}} \ldots (8.)$$

$$\cos\kappa = \pm \frac{(\cos\epsilon_{,} - \cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,,}})\sin\beta_{,} + (\cos\epsilon_{,,} - \cos\epsilon_{,}\cos\overline{\beta_{,,}-\beta_{,}})\cos\beta}{\sin\overline{\beta_{,,}-\beta_{,}}\sqrt{\cos^2\epsilon_{,} - 2\cos\epsilon_{,}\cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,}} + \cos^2\epsilon_{,,}}} \ldots (9.)$$

$$\cot\lambda = \pm \frac{2\,\mathrm{R}}{\sqrt{\cos^2\epsilon_{,} - 2\cos\epsilon_{,}\cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,}} + \cos^2\epsilon_{,,}}} \ldots\ldots (10.)$$

The expressions in (7, 8, 9), though perfectly symmetrical, are not the simplest that can be given to those quantities. For, taking the numerator of (7), and expanding the cosines, and multiplying out,

$$(\cos\epsilon_{,} - \cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,}})\cos\beta_{,} + (\cos\epsilon_{,,} - \cos\epsilon_{,}\cos\overline{\beta_{,,}-\beta_{,}})\cos\beta_{,}$$

$$= \cos\epsilon_{,}\{\cos\beta_{,} - \cos^2\beta_{,,}\cos\beta_{,} - \cos\beta_{,,}\sin\beta_{,,}\sin\beta_{,}\} + \cos\epsilon_{,,}\{\cos\beta_{,,} - \cos\beta_{,,}\cos^2\beta_{,} - \cos\beta_{,}\sin\beta_{,}\sin\beta_{,,}\}$$

$$= \cos\epsilon_{,}\{\sin^2\beta_{,,}\cos\beta_{,} - \sin\beta_{,,}\sin\beta_{,}\cos\beta_{,,}\} + \cos\epsilon_{,,}\{\sin^2\beta_{,}\cos\beta_{,,} - \sin\beta_{,,}\sin\beta_{,}\cos\beta_{,}\}$$

$$= \{\cos\epsilon_{,}\sin\beta_{,,} - \cos\epsilon_{,,}\sin\beta_{,}\}\{\sin\beta_{,,}\cos\beta_{,} - \sin\beta_{,}\cos\beta_{,,}\}$$

$$= \{\cos\epsilon_{,}\sin\beta_{,,} - \cos\epsilon_{,,}\sin\beta_{,}\}\sin\overline{\beta_{,,}-\beta_{,}} \ldots\ldots (11)$$

In like manner, for the denominator of equation (7), we have

$$\cos\epsilon_{,}(\sin\beta_{,} - \sin^2\beta_{,,}\sin\beta_{,} - \cos\beta_{,,}\sin\beta_{,,}\cos\beta_{,}) + \cos\epsilon_{,,}(\sin\beta_{,,} - \sin^2\beta_{,}\sin\beta_{,,} - \sin\beta_{,}\cos\beta_{,}\cos\beta_{,,})$$

$$= \cos\epsilon_{,}(\sin\beta_{,}\cos^2\beta_{,,} - \cos\beta_{,}\cos\beta_{,,}\sin\beta_{,,}) + \cos\epsilon_{,,}(\sin\beta_{,,}\cos^2\beta_{,} - \sin\beta_{,}\cos\beta_{,}\cos\beta_{,,})$$

$$= -(\cos\epsilon_{,}\cos\beta_{,,} - \cos\epsilon_{,,}\cos\beta_{,,})\sin\overline{\beta_{,,}-\beta_{,}} \ldots\ldots (12.)$$

The equations (7, 8, 9) then become, respectively,

$$\tan\kappa = \frac{\cos\epsilon_{,}\sin\beta_{,,} - \cos\epsilon_{,,}\sin\beta_{,}}{\cos\epsilon_{,}\cos\beta_{,,} - \cos\epsilon_{,,}\cos\beta_{,}} \ldots\ldots (13.)$$

$$\sin\kappa = \mp \frac{\cos\epsilon_{,}\sin\beta_{,,} - \cos\epsilon_{,,}\sin\beta_{,}}{\sqrt{\cos^2\epsilon_{,} - 2\cos\epsilon_{,}\cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,}} + \cos^2\epsilon_{,,}}} \ldots\ldots (14.)$$

$$\cos\kappa = \mp \sqrt{\frac{\cos\epsilon_{,}\cos\beta_{,,} - \cos\epsilon_{,,}\cos\beta_{,}}{\cos^2\epsilon_{,} - 2\cos\epsilon_{,}\cos\epsilon_{,,}\cos\overline{\beta_{,,}-\beta_{,}} + \cos^2\epsilon_{,,}}} \ldots\ldots (15.)$$

Now, in the general equation of the circle, (I.) of this paper, put the values of $\tan\lambda$, $\cos\kappa$, and $\sin\kappa$, then there will result the final equation

$$\cot\phi = -\frac{1}{2\,\mathrm{R}}\{\cos\theta(\cos\epsilon_{,}\cos\beta_{,,} - \cos\epsilon_{,,}\cos\beta_{,}) + \sin\theta(\cos\epsilon_{,}\sin\beta_{,,} - \cos\epsilon_{,,}\sin\beta_{,})\} \ldots (16.)$$

Cor. If $\epsilon_{,}$ and $\epsilon_{,,}$ be right angles, we shall have $\cot\phi = 0$, whatever θ may be, as we otherwise know it should be, the circle sought being then the equator.

VI.

Required the co-ordinates of the points of intersection of two given great circles.

Denote the given circles by

$$\cot\phi = -\tan\lambda_{,}\cos(\theta - \kappa_{,}) \quad \text{..........(1.)}$$

$$\cot\phi = -\tan\lambda_{,,}\cos(\theta - \kappa_{,,}) \quad \text{..........(2.)}$$

From these we have

$$\frac{\tan\lambda_{,}}{\tan\lambda_{,,}} = \frac{\cos(\theta-\kappa_{,,})}{\cos(\theta-\kappa_{,})} = \frac{\cos\kappa_{,,} + \sin\kappa_{,,}\tan\theta}{\cos\kappa_{,} + \sin\kappa_{,}\tan\theta}, \text{ or}$$

$$\tan\theta = -\frac{\tan\lambda_{,}\cos\kappa_{,} - \tan\lambda_{,,}\cos\kappa_{,,}}{\tan\lambda_{,}\sin\kappa_{,} - \tan\lambda_{,,}\sin\kappa_{,,}} \quad \text{..........(3.)}$$

$$\sin\theta = \mp\frac{\tan\lambda_{,}\cos\kappa_{,} - \tan\lambda_{,,}\cos\kappa_{,,}}{\sqrt{\tan^2\lambda_{,} - 2\tan\lambda_{,}\tan\lambda_{,,}\cos(\kappa_{,,}-\kappa_{,}) + \tan^2\lambda_{,,}}} \quad \text{..........(4.)}$$

$$\cos\theta = \mp\frac{\tan\lambda_{,}\sin\kappa_{,} - \tan\lambda_{,,}\sin\kappa_{,,}}{\sqrt{\tan^2\lambda_{,} - 2\tan\lambda_{,}\tan\lambda_{,,}\cos(\kappa_{,,}-\kappa_{,}) + \tan\lambda_{,,}}} \quad \text{..........(5.)}$$

Again, from equations (1, 2) we find

$$\theta = \cos^{-1}(-\cot\phi\cot\lambda_{,}) + \kappa_{,}$$

$$\theta = \cos^{-1}(-\cot\phi\cot\lambda_{,,}) + \kappa_{,,}$$

and therefore,

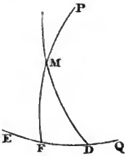
$$\cos^{-1}(-\cot\phi\cot\lambda_{,}) - \cos^{-1}(\cot\phi\cot\lambda_{,,}) = \kappa_{,,} - \kappa_{,}$$

Or taking cosine-function of both sides, transposing and squaring, we get

$$\cot\phi = \frac{\pm\sin\overline{\kappa_{,,}-\kappa_{,}}}{\sqrt{\cot^2\lambda_{,} - 2\cot\lambda_{,}\cot\lambda_{,,}\cos\overline{\kappa_{,,}-\kappa_{,}} + \cot^2\lambda_{,,}}} \quad \text{..........(6.)}$$

VII.

To find the angle made by a given great circle with a given meridian.



Let the given circle and the given meridian be M F and M D, and denote them by the equations

$$\cot \phi = - \tan \lambda \cos (\theta - \kappa) \quad \text{.......(1.)}$$

$$\theta = \beta_{,} \quad \text{.......(2.)}$$

When they intersect, we have

$$\cot \phi = - \tan \lambda \cos (\beta_{,} - \kappa) \quad \text{.......(3.)}$$

Again, when the given great circle intersects the equator, we have

$$\cos (\theta - \kappa) = 0, \text{ or } \theta = \frac{\pi}{2} + \kappa = \widehat{FD}, \text{ or}$$

$$\widehat{FD} = \frac{\pi}{2} + \kappa - \beta_{,} \quad \text{.......(4.)}$$

But, by Napier, $\sin \widehat{MF} = \tan \widehat{FD} \cot FMD$, or putting the angle $= \epsilon_{,}$, we have

$$\begin{aligned} \cot \epsilon_{,} = \frac{\sin \widehat{MF}}{\tan \widehat{FD}} &= \frac{\cos \phi}{-\cot (\kappa - \beta_{,})} \\ &= \frac{\cos \cot^{-1} (- \tan \lambda \cos \overline{\beta_{,} - \kappa})}{\cot (\kappa - \beta_{,})} \\ &\left.\begin{aligned} &= \pm \frac{\tan \lambda \sin \overline{\kappa - \beta_{,}}}{\sqrt{1 + \tan^2 \lambda \cos^2 \overline{\kappa - \beta_{,}}}} \\ &= \pm \frac{\sin \overline{\kappa - \beta_{,}} \sin \lambda}{\sqrt{1 - \sin^2 \lambda \sin^2 \overline{\kappa - \beta_{,}}}} \end{aligned}\right\} \quad \text{.......(5.)} \end{aligned}$$

Also, from (5) we readily obtain the sine and cosine of $\epsilon_,$; viz.

$$\cos \epsilon_, = \pm \sin \lambda \sin \overline{\kappa - \beta_,} \text{.......................................} (6.)$$

$$\sin \epsilon_, = \pm \sqrt{1 - \sin^2 \lambda \sin^2 \overline{\kappa - \beta_,}} \text{..................................} (7.)$$

VIII.

To determine the angle contained by two given great circles.

Let the circles be denoted by

$$\cot \phi = - \tan \lambda_, \cos (\theta - \kappa_,) \text{.....................................} (1.)$$

$$\cot \phi = - \tan \lambda_{,,} \cos (\theta - \kappa_{,,}) \text{...................................} (2.)$$

The angles made by each of these with the meridian through their points of intersection, are found, by Art. VII., putting κ for the longitude or abscissa of intersection.

For from (VII, 6) we have

$$\epsilon_, = \cos^{-1} \sin \lambda_, \sin (\kappa - \kappa_,) \text{...} (3.)$$

$$\epsilon_{,,} = \cos^{-1} \sin \lambda_{,,} \sin (\kappa - \kappa_{,,}) \text{.......................................} (4.)$$

Hence

$$\cos^{-1} \sin \lambda_, \sin (\kappa - \kappa_,) - \cos^{-1} \sin \lambda_{,,} \sin (\kappa - \kappa_{,,}) = \epsilon_, - \epsilon_{,,} \text{...........} (5.)$$

or taking cosine function, and reducing the result, we should obtain a complicated expression for $\cos (\epsilon_, - \epsilon_{,,})$, involving a radical. The better way, then, if this method be adopted at all, is to compute $\epsilon_,$ and $\epsilon_{,,}$ separately from (3 and 4). But there is a more direct method of effecting this object, at least in an analytical point of view. It is as follows.

The distance of the centres of two great circles on the sphere is the measure of the inclination of those circles. If, therefore, we denote by δ that distance, and recur to our equations (1 and 2), we shall have

$$\cos \delta = \cos \lambda_, \cos \lambda_{,,} + \sin \lambda_, \sin \lambda_{,,} \cos \overline{\kappa_{,,} - \kappa_,} \text{................................} (6.)$$

and δ is the angle made by the two circles.

Cor. 1. If $\lambda_{,,} = \frac{\pi}{2}$, then one of these circles is a meridian, and we obtain

$$\cos \delta = \sin \lambda_, \cos \overline{\kappa_{,,} - \kappa_,} \text{................................ ....} (7.)$$

which agrees with the expression obtained in (VII, 6), bearing in mind that δ is put for $\epsilon_{,}$ and $\kappa_{,}$ for $\beta_{,}$, and $\kappa_{,,}$ for $\beta_{,} + \frac{\pi}{2}$.

Cor. 2. If both be meridians, then $\cos\delta = \cos\overline{\kappa_{,,} - \kappa_{,}}$, that is, the arc of the equator cut off by them measures their inclination, as we know it should.

IX.

To find the equation of a great circle having a common intersection with two given circles.

$$\text{Let } \cos\rho_{,} = \cos\lambda_{,}\cos\phi + \sin\lambda_{,}\sin\phi\cos\overline{\theta - \kappa_{,}} \quad \ldots\ldots\ldots\ldots \quad (1.)$$

$$\cos\rho_{,,} = \cos\lambda_{,,}\cos\phi + \sin\lambda_{,,}\sin\phi\cos\overline{\theta - \kappa_{,,}} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

be the given circles; and let the great circle sought be denoted by

$$\cot\phi = -\tan\lambda\cos\overline{\theta - \kappa} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3.)$$

in which we have to determine λ and κ.

Multiply (1) by $\cos\lambda_{,}$ and (2) by $\cos\lambda_{,,}$ and subtract: then

$$\sin\phi = \frac{\sin\lambda_{,}\cos\lambda_{,,}\cos\overline{\theta - \kappa_{,}} - \cos\lambda_{,}\sin\lambda_{,,}\cos\overline{\theta - \kappa_{,,}}}{\cos\rho_{,}\cos\lambda_{,,} - \cos\rho_{,,}\cos\lambda_{,}} \quad \ldots\ldots \quad (4.)$$

Multiply (1) by $\sin\lambda_{,,}\cos\overline{\theta - \kappa_{,,}}$ and (2) by $\sin\lambda_{,}\cos\overline{\theta - \kappa_{,}}$ and subtract; which gives

$$\cos\phi = \frac{\cos\rho_{,}\sin\lambda_{,,}\cos\overline{\theta - \kappa_{,,}} - \cos\rho_{,,}\sin\lambda_{,}\cos\overline{\theta - \kappa_{,}}}{\cos\lambda_{,}\sin\lambda_{,,}\cos\overline{\theta - \kappa''} - \cos\lambda_{,,}\sin\lambda_{,}\cos\overline{\theta - \kappa_{,}}} \quad \ldots \quad (5.)$$

Divide (5) by (4); then we find the equation of the sought circle:

$$\cot\phi = -\frac{\cos\rho_{,}\sin\lambda_{,,}\cos\overline{\theta - \kappa_{,,}} - \cos\rho_{,,}\sin\lambda_{,}\cos\overline{\theta - \kappa_{,}}}{\cos\rho_{,}\cos\lambda_{,,} - \cos\rho_{,,}\cos\lambda_{,}} \quad \ldots \quad (6.)$$

Or, to accommodate it to the form (3), we must expand the cosines of this, and we find

$$\cot\phi = -\frac{(\cos\rho_,\sin\lambda_{,,}\cos\kappa_{,,}-\cos\rho_{,,}\sin\lambda_,\cos\kappa_,)\cos\theta+(\cos\rho_,\sin\lambda_{,,}\sin\kappa_{,,}-\cos\rho_{,,}\sin\lambda_,\sin\kappa_,)\sin\theta}{\cos\rho_,\cos\lambda_{,,}-\cos\rho_{,,}\cos\lambda_,} \quad\ldots (7.)$$

$$\text{Hence, } \tan\kappa = \frac{\cos\rho_,\sin\lambda_{,,}\sin\kappa_{,,}-\cos\rho_{,,}\sin\lambda_,\sin\kappa_,}{\cos\rho_,\sin\lambda_{,,}\cos\kappa_{,,}-\cos\rho_{,,}\sin\lambda_,\cos\kappa_,} \quad\ldots\ldots\ldots\ldots (8.)$$

The radius to which this is referred, is

$$= \{\cos^2\rho_,\sin^2\lambda_{,,}-2\cos\rho_,\cos\rho_{,,}\sin\lambda_,\sin\lambda_{,,}\cos\overline{\kappa_{,,}-\kappa_,}+\cos^2\rho_{,,}\sin^2\lambda_,\}^{\frac{1}{2}} \quad\ldots\ldots(9.)$$

$$\text{And } \tan\lambda = \pm\frac{\{\cos^2\rho_,\sin^2\lambda_{,,}-2\cos\rho_,\cos\rho_{,,}\sin\lambda_,\sin\lambda_{,,}\cos\overline{\kappa_{,,}-\kappa_,}+\cos^2\rho_{,,}\sin^2\lambda'\}^{\frac{1}{2}}}{\cos\rho_,\cos\lambda_{,,}-\cos\rho_{,,}\cos\lambda_,} \quad ..(10.)$$

The circle is hence completely determined.

X.

To find the equation of a great circle which passes through a given point $a_,\ \beta_,$, and is perpendicular to a given great circle.

$$\text{Let } \cot\phi = -\tan\lambda_,\cos(\theta-\kappa_,) \quad\ldots\ldots\ldots\ldots (1.)$$

be the given circle; and let that sought be denoted by

$$\cot\phi = -\tan\lambda\cos(\theta-\kappa) \quad\ldots\ldots\ldots\ldots (2.)$$

But this passes through $a_,\ \beta_,$, and therefore, also,

$$\cot a_, = -\tan\lambda\cos(\beta_,-\kappa) \quad\ldots\ldots\ldots\ldots (3.)$$

Also the inclination of two circles being

$$\cos\epsilon_, = \cos\lambda\cos\lambda_, + \sin\lambda\sin\lambda_,\cos(\kappa'-\kappa) \quad\ldots\ldots (4.)$$

but in the present case they are perpendicular, $\epsilon_, = \frac{\pi}{2}$, and hence (4) becomes

$$\cot\lambda = -\tan\lambda_,\cos\overline{\kappa-\kappa_,} \quad\ldots\ldots\ldots\ldots (5.)$$

Between (3) and (5) we have to determine κ and λ.

We have, by division,

$$\frac{\tan\lambda_,}{\tan a_,} = \frac{\cos\overline{\beta_,-\kappa}}{\cos\overline{\kappa_,-\kappa}} = \frac{\cos\beta_,+\sin\beta_,\tan\kappa}{\cos\kappa_,+\sin\kappa_,\tan\kappa}, \text{ from which}$$

$$\tan\kappa = -\frac{\tan a_,\cos\beta_,-\tan\lambda_,\cos\kappa_,}{\tan a_,\sin\beta_,-\tan\lambda_,\sin\kappa_,} \quad\ldots\ldots\ldots\ldots (6.)$$

$$\sin\kappa = \mp \frac{\tan\alpha_{,} \cos\beta_{,} - \tan\lambda_{,} \cos\kappa_{,}}{\sqrt{\tan^2\alpha_{,} - 2\tan\alpha_{,} \tan\lambda_{,} \cos\overline{\beta_{,} - \kappa_{,}} + \tan^2\lambda_{,}}} \quad \ldots\ldots (7)$$

$$\cos\kappa = \pm \frac{\tan\alpha_{,} \sin\beta_{,} - \tan\lambda_{,} \sin\kappa_{,}}{\sqrt{\tan^2\alpha_{,} - 2\tan\alpha_{,} \tan\lambda_{,} \cos\overline{\beta_{,} - \kappa_{,}} + \tan^2\lambda_{,}}} \quad \ldots\ldots (8)$$

Again, $\cot\lambda = -\tan\lambda_{,} (\cos\kappa \cos\kappa_{,} + \sin\kappa \sin\kappa_{,})$

$$= -\frac{\tan\lambda_{,} \{\cos\kappa_{,} (\tan\alpha_{,} \sin\beta_{,} - \tan\lambda_{,} \sin\kappa_{,}) - \sin\kappa_{,} (\tan\alpha_{,} \cos\beta_{,} - \tan\lambda_{,} \cos\kappa_{,})\}}{\sqrt{\tan^2\alpha_{,} - 2\tan\alpha_{,} \tan\lambda_{,} \cos\overline{\beta_{,} - \kappa_{,}} + \tan^2\lambda_{,}}}$$

$$= \pm \frac{\tan\lambda_{,} \tan\alpha_{,} \sin\overline{\beta - \kappa_{,}}}{\sqrt{\tan^2\alpha_{,} - 2\tan\alpha_{,} \tan\lambda_{,} \cos\overline{\beta_{,} - \kappa_{,}} + \tan^2\lambda_{,}}} \quad \ldots\ldots\ldots\ldots (9)$$

The equation of the circle is, therefore,

$$\cot\phi = \mp \frac{\cot\lambda_{,} \cot\alpha_{,}}{\sin\overline{\beta_{,} - \kappa_{,}}} \{\cos\theta (\tan\alpha_{,} \sin\beta_{,} - \tan\lambda_{,} \sin\kappa_{,}) - \sin\theta (\tan\alpha_{,} \cos\beta_{,} - \tan\lambda_{,} \cos\kappa_{,})\} \ldots (10)$$

Cor. 1. If $\alpha_{,} = \lambda_{,}$ and $\beta_{,} = \kappa_{,}$, or which is the same thing, if the given point coincides with the centre of circle (1), we shall have

$$\cot\phi = \frac{0}{0} \quad \ldots\ldots\ldots\ldots\ldots (11)$$

indicating that the circle is *indeterminate*,* as from other considerations we know it should be.

Cor. 2. The same result would have been obtained by considering that the great circle, which is perpendicular to another, passes through its centre, and therefore that the great circle sought passes through $\alpha_{,} \beta_{,}$ and $\lambda_{,} \kappa_{,}$. Hence by (IV.) we should have a result similar to this one; and, indeed, dividing both numerator and denominator of (9, IX.) by $\tan\lambda_{,} \tan\alpha_{,}$, we shall have the same form as well as the same value of $\cot\lambda$, as is furnished

* It is indeterminate, because the equation (11) is fulfilled by the co-efficients, which should determine the circle,—not from a particular value assumed by the variable θ, which would merely indicate an extraneous factor, that might be eliminated by differentiation.

by (8, IV.) And carrying $\cot a_, \cot \lambda_,$ into the term within the vinculum in (10, IX.), we shall have the same form as that given in (9, IV.)

Cor. 3. If $a_, = \lambda_,$, not taking $\beta_, = \kappa_,$, we shall have (10) converted into

$$\cot \phi = \pm \frac{\cot^2 a_, \tan a_,}{\sin \overline{\beta_, - \kappa_,}} \{\cos \theta (\sin \beta_, - \sin \kappa_,) - \sin \theta (\cos \beta_, - \cos \kappa_,)\}$$

$$= \pm \frac{2 \cot a_, \sin \frac{\beta_, - \kappa_,}{2}}{\sin \overline{\beta_, - \kappa_,}} \cos\left(\frac{\beta_, + \kappa_,}{2} - \theta\right)$$

$$= \pm \frac{\cot a_,}{\cos \frac{\beta_, - \kappa_,}{2}} \cos\left(\frac{\beta_, + \kappa_,}{2} - \theta\right) \quad \text{.................} \quad (12)$$

which is the equation of a circle perpendicular to the given one, and such that the centres have the same latitude.

Cor. 4. If we seek that one of *this class of circles* which belongs to the limit of $\beta_, - \kappa_,$, we shall find it by putting $\beta_, = \kappa_,$ in equation (12). This will give

$$\cos \frac{\beta_, - \kappa_,}{2} = 1, \text{ and } \cos\left(\frac{\beta_, + \kappa_,}{2} - \theta\right) = \cos (\beta_, - \theta),$$

and the equation is,

$$\cot \phi = \pm \cot a_, \cos (\beta_, - \theta) \quad \text{................} \quad (13)$$

Cor. 5. If we had taken originally $\beta_, = \kappa_,$, establishing no other relation amongst the constants, for the present, we should have had

$$\cot \phi = \mp \frac{\cot \lambda_, \cot a_,}{\sin 0^\circ} \{\cos \theta \sin \beta_, (\tan a_, - \tan \lambda_,) - \sin \theta \cos \beta_, (\tan a_, - \tan \lambda_,)\}$$

$$= \mp \frac{\cot \lambda_, \cot a_, \sin (\beta_, - \theta) (\tan a_, - \tan \lambda_,)}{\sin 0^\circ} \quad \text{..................................} \quad (14$$

This equation gives rise to the following remarks:

1*mo*, The denominator being essentially zero, if the four factors in the numerators be finite, the value of $\cot \phi$ is necessarily *infinite*, and hence

$\phi = 0°$, or $\phi = \pi$. That is, in this case, the two poles of the equator fulfil the conditions, and these alone.

2do, If we take that particular meridian $\theta = \beta_{,}$, we shall have

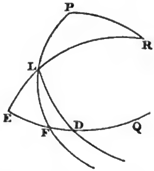
$$\cot \phi = \mp \frac{0}{0} \quad \text{..} \quad (14)$$

which shows that ϕ may *be of any value*, or, in other words, the circle sought is the meridian thus determined.

3tio, If, instead of this, we take $\alpha_{,} = \lambda_{,}$, or rather $\tan \alpha_{,} = \tan \lambda_{,}$, we have also $\cos \phi = \frac{0}{0}$; or ϕ may be of any value, whatever θ may be. That is, the circle itself is *indeterminate*, being subjected in reality to a condition less in number than is necessary to define it.

XI.

To find the equation of a great circle which makes with a given meridian at a given point in that meridian a given angle.



Let $\epsilon = \mathrm{FLD}$ be the given angle, α, β the given point L, and LD the circle sought.

Let the equation be assumed,

$$\cot \phi = -\tan \lambda \cos (\theta - \kappa) \quad \text{................} \quad (1)$$

Now, when $\theta = \beta$, we have

$$\cot \alpha = -\tan \lambda \cos (\beta - \kappa) \quad \text{................} \quad (2)$$

Draw LR perpendicular to LD, and make it $= \frac{\pi}{2}$; and join PR. Then R is the centre of the circle LD, and we are required to find the co-ordinates of that point.

Now $\text{PLR} = \frac{\pi}{2} - \epsilon$, and PLR is a quandrantal triangle. Hence

$$\cos\lambda = \sin\alpha \sin\epsilon \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3)$$

$$\text{and } \cos \text{PLB, or } \cos(\kappa - \beta) = -\cot\alpha \cot\lambda \quad \ldots\ldots \quad (4)$$

$$\text{or } \kappa = \beta - \cos^{-1}(-\cot\alpha \cot\lambda) \quad \ldots\ldots \quad (5)$$

From (3) we obtain, $\cot\lambda = \cot\cos^{-1}(\sin\alpha \sin\epsilon)$

$$\left.\begin{aligned} &= \frac{\sin\alpha \sin\epsilon}{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}} \\ \text{and } \therefore \tan\lambda &= \frac{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}}{\sin\alpha \sin\epsilon} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (6)$$

It will hence follow, that

$$\begin{aligned} \cos\kappa &= -\cos\beta \cot\alpha \cot\lambda + \sin\beta \sqrt{1 - \cot^2\alpha \cot^2\lambda} \\ &= \frac{-\cos\beta \cos\alpha \sin\epsilon}{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}} + \frac{\sin\beta \cos\epsilon}{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}} \\ &= \frac{\sin\beta \cos\epsilon - \sin\epsilon \cos\beta \cos\alpha}{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}} \quad \ldots\ldots\ldots \quad (7) \end{aligned}$$

$$\sin\kappa = \sqrt{1 - \cos^2\kappa} = \frac{\cos\epsilon \cos\beta + \sin\epsilon \sin\beta \cos\alpha}{\sqrt{1 - \sin^2\alpha \sin^2\epsilon}} \quad \ldots\ldots \quad (8)$$

For $\sin\kappa$, $\cos\kappa$, $\tan\lambda$, in equation (1), write the values furnished by (6, 7, 8); then,

$$\cot\phi = -\operatorname{cosec}\alpha \operatorname{cosec}\epsilon \left\{ \begin{aligned} &\cos\theta (\sin\beta \cos\epsilon - \sin\epsilon \cos\beta \cos\alpha) \\ &+ \sin\theta (\cos\theta \cos\epsilon + \sin\epsilon \sin\beta \cos\alpha) \end{aligned} \right\} \quad \ldots\ldots\ldots \quad (9)$$

XII.

To describe a great circle through a given point, which shall make a given angle with a given great circle.

Let the given point N have $\alpha_{,}\ \beta_{,}$ for co-ordinates, and the given circle M L be

$$\cot\phi = -\tan\lambda_{,}\cos(\theta - \kappa_{,}) \dots\dots (1.)$$

Then also the inclination being given, $= \epsilon_{,}$, and the equation of the circle sought, assumed to be

$$\cot\phi = -\tan\lambda\cos(\theta - \kappa) \dots\dots (2.)$$

we shall have (VIII, 6) also the equation

$$\cos\epsilon_{,} = \cos\lambda\cos\lambda_{,} + \sin\lambda\sin\lambda_{,}\cos(\kappa - \kappa_{,}) \dots\dots (3.)$$

Also, because (2) passes through $\alpha_{,}\ \beta_{,}$, we have

$$\cot\alpha_{,} = -\tan\lambda\cos(\beta_{,} - \kappa) \dots\dots (4.)$$

We are required to find κ and λ from equations (3) and (4).

We may put the equation (4) under either of the forms,

$$\left.\begin{array}{l} \cos\lambda = -\sin\lambda\tan\alpha_{,}\cos(\beta_{,} - \kappa) \\ \sin\lambda = -\cos\lambda\cot\alpha_{,}\sec(\beta_{,} - \kappa) \end{array}\right\} \dots\dots (5.)$$

Putting these successively in (3), we obtain, after slight reduction,

$$\sin\lambda = -\frac{\cos\epsilon_{,}\cos\alpha_{,}}{\cos\lambda_{,}\sin\alpha_{,}\cos\overline{\beta_{,} - \kappa} - \cos\alpha_{,}\sin\lambda_{,}\cos\overline{\kappa_{,} - \kappa}}$$

$$= -\frac{\cos\epsilon_{,}\cos\alpha_{,}}{(\cos\lambda_{,}\sin\alpha_{,}\cos\beta_{,} - \cos\alpha_{,}\sin\lambda_{,}\cos\kappa_{,})\cos\kappa + (\cos\lambda_{,}\sin\alpha_{,}\sin\beta_{,} - \cos\alpha_{,}\sin\lambda_{,}\sin\kappa_{,})\sin\kappa} \dots (6.)$$

$$\cos\lambda = \frac{\cos\epsilon_{,}\sin\alpha_{,}\cos\overline{\beta_{,} - \kappa}}{\cos\lambda_{,}\sin\alpha_{,}\cos\overline{\beta_{,} - \kappa} - \cos\alpha_{,}\sin\lambda_{,}\cos\overline{\kappa_{,} - \kappa}}$$

$$= \frac{\cos\epsilon_{,}\sin\alpha_{,}\cos\beta_{,}\cos\kappa + \cos\epsilon_{,}\sin\alpha_{,}\sin\beta_{,}\sin\kappa}{(\cos\lambda_{,}\sin\alpha_{,}\cos\beta_{,} - \cos\alpha_{,}\sin\lambda_{,}\cos\kappa_{,})\cos\kappa + (\cos\lambda_{,}\sin\alpha_{,}\sin\beta_{,} - \cos\alpha_{,}\sin\lambda_{,}\sin\kappa_{,})\sin\kappa} \dots (7.)$$

But $\cos^2\lambda + \sin^2\lambda = 1 \dots\dots (8.)$

Or putting in (8) the values of $\sin\lambda$, $\cos\lambda$, from (6, 7), we have an equation of the form

$$\left.\begin{array}{l} A^2 + B^2\cos^2\kappa + 2\,BC\cos\kappa\sin\kappa + C^2\sin^2\kappa \\ \quad - D^2\cos^2\kappa - 2\,DE\cos\kappa\sin\kappa - E^2\sin^2\kappa \end{array}\right\} = 0 \ldots\ldots\ldots (9.)$$

$$\left.\begin{array}{rcl} \text{Where } A & = & -\cos\epsilon,\cos\alpha, \\ B & = & \cos\epsilon,\sin\alpha,\cos\beta, \\ C & = & \cos\epsilon,\sin\alpha,\sin\beta, \\ D & = & \cos\lambda,\sin\alpha,\cos\beta, -\cos\alpha,\sin\lambda,\cos\kappa, \\ E & = & \cos\lambda,\sin\alpha,\sin\beta, -\cos\alpha,\sin\lambda,\sin\kappa, \end{array}\right\} \ldots\ldots\ldots\ldots (10.)$$

$$\text{or } \frac{(A^2+B^2-D^2)}{DE-BC} - \frac{\overline{B^2-D^2}-\overline{C^2-E^2}}{DE-BC}\sin^2\kappa = 2\sin\kappa\cos\kappa, \text{ or}$$

$$G - H\sin^2\kappa = 2\sin\kappa\cos\kappa\,;$$

Or squaring, transposing, &c.

$$G^2 - 2\,(GH+2)\sin^2\kappa + (H^2+4)\sin^4\kappa = 0 \ldots\ldots\ldots\ldots (11.)$$

$$\sin^2\kappa = \frac{GH + 2 \pm 2\sqrt{1+GH-G^2}}{H^2+4} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12.)$$

$$\cos^2\kappa = \frac{H^2 - GH + 2 \mp 2\sqrt{1+GH-G^2}}{H^2+4} \ldots\ldots\ldots\ldots (13.)$$

Or putting for G and H their values, we have

$$\sin^2\kappa = \frac{(A^2+B^2-D^2)(\overline{B^2-D^2}-\overline{C^2-E^2}) + 2(DE-BC)^2 \pm 2(DE-BC)\sqrt{(DE-BC)^2-(A^2+C^2-E^2)(\overline{B^2-D^2}-\overline{C^2-B^2})}}{(\overline{B^2-D^2}-\overline{C^2-E^2})^2 + 4\,(DE-BC)^2} \ldots (14.)$$

$$\cos^2\kappa = \frac{-(A^2+C^2-E^2)(\overline{B^2-D^2}-\overline{C^2-E^2}) + 2(DE-BC)^2 \mp 2(DE-BC)\sqrt{(DE-BC)^2-(A^2+C^2-E^2)(\overline{B^2-D^2}-\overline{C^2-E^2})}}{(\overline{B^2-D^2}-\overline{C^2-E^2})^2 + 4\,(DE-BC)^2} \ldots (15.)$$

These expressions in the general solution, and in their present form, are very complex. If, however, facility of calculation be aimed at, they may be simplified in various ways. But as my object in this paper is a totally different one,—that of obtaining analytical formulæ to express the constants of an indeterminate geometrical problem, which are only implicitly given,—it is unnecessary to dwell upon the question in any other point of view. Instead, therefore, of attempting any transformation, I shall conclude the article by a remark on the signification of the solution just obtained.

As, from any point on the sphere, we may proceed in *four* different directions so as to cut the given circle under a given angle, still so related that one pair will form adjacent arcs of one great circle, and the other two of another, so there will be two great circles which fulfil the condition required. Each of these has two centres diametrically opposite on the sphere; and the values of $\sin\kappa$ given by extracting the root of (14), and the values of $\cos\kappa$ from (15) refer to the longitudes of these four centres, and determine them. The values of $\sin\lambda$ and $\cos\lambda$ can now be determined from (6, 7), and the question is completely solved.

When the given angle is a right one, $\cos\epsilon_{,} = 0_{,}$, and hence $A = B = C = 0$, and the radical vanishes, as well as certain other of the terms involved in $\cos\kappa$ and $\sin\kappa$. We thus get

$$\left.\begin{aligned} \sin^2\kappa &= \frac{E^2(E^2 - D^2) + 2E^2 - D^2}{4DE^2 + (E^2 - D^2)^2} = \frac{E^2}{E^2 + D^2} \\ \sin^2\kappa &= \frac{-D^2(-\overline{D^2 - E^2}) + 2D^2 - E^2}{4D^2E^2 + (E^1 - D^2)^2} = \frac{D^2}{E^2 + D^2} \end{aligned}\right\} \quad \ldots\ldots \quad (16.)$$

agreeing with the result obtained in (IX, 7, 8), where this particular case was the subject of examination.

XIII.

To find the equation of a circle through three given points on the sphere.

Let the circle be denoted by

$$\cos\rho = \cos\lambda\cos\phi + \sin\lambda\sin\phi\cos(\theta - \kappa) \ldots\ldots\ldots\ldots (1.)$$

and the three points be $\alpha\beta$, $\alpha_{,}\beta_{,}$, and $\alpha_{,,}\beta_{,,}$. Then we have, because (1) passes through these three points, to find ρ, κ, λ, from the three following equations:

$$\cos\rho = \cos\lambda\cos\alpha + \sin\lambda\sin\alpha\cos(\beta - \kappa) \ldots\ldots\ldots\ldots (2.)$$

$$\cos\rho = \cos\lambda\cos\alpha_{,} + \sin\lambda\sin\alpha_{,}\cos(\beta_{,} - \kappa) \ldots\ldots\ldots\ldots (3.)$$

$$\cos\rho = \cos\lambda\cos\alpha_{,,} + \sin\lambda\sin\alpha_{,,}\cos(\beta_{,,} - \kappa) \ldots\ldots\ldots\ldots (4.)$$

Subtract (3) from (2), and (4) from (3); then we have

$$\cot\lambda\,(\cos\alpha - \cos\alpha_{,}) = \sin\alpha_{,}\cos(\beta_{,} - \kappa) - \sin\alpha\cos(\beta - \kappa) \ldots\ldots (5.)$$

$$\cot\lambda\,(\cos\alpha - \cos\alpha_{,,}) = \sin\alpha_{,,}\cos(\beta_{,,} - \kappa) - \sin\alpha\cos(\beta - \kappa) \ldots (6.)$$

Expand these, and arrange them according to $\cos\kappa$ and $\sin\kappa$. Then

$$\cot\lambda\,(\cos\alpha - \cos\alpha_{,}) = (\sin\alpha_{,}\cos\beta_{,} - \sin\alpha\cos\beta)\cos\kappa + (\sin\alpha_{,}\sin\beta_{,} - \sin\alpha\sin\beta)\sin\kappa \ldots\ldots (7.)$$

$$\cot\lambda\,(\cos\alpha - \cos\alpha_{,,}) = (\sin\alpha_{,,}\cos\beta_{,,} - \sin\alpha\cos\beta)\cos\kappa + (\sin\alpha_{,,}\sin\beta_{,,} - \sin\alpha\sin\beta)\sin\kappa \ldots\ldots (8.)$$

Resolving these (7, 8) with respect to $\sin\kappa$ and $\cos\kappa$, we have

$$\sin\kappa = \left[\frac{\cos\alpha\,(\sin\alpha_{,,}\cos\beta_{,,} - \sin\alpha_{,}\cos\beta_{,}) + \cos\alpha_{,}\,(\sin\alpha\cos\beta - \sin\alpha_{,,}\cos\beta_{,,}) + \cos\alpha_{,,}\,(\sin\alpha_{,}\cos\beta_{,} - \sin\alpha\cos\beta)}{\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,} - \beta_{,,}} + \sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,} - \beta} + \sin\alpha\sin\alpha_{,}\sin\overline{\beta - \beta_{,}}}\right]\cot\lambda \quad (9.)$$

$$\cos\kappa = -\left[\frac{\cos\alpha\,(\sin\alpha_{,,}\sin\beta_{,,} - \sin\alpha_{,}\sin\beta_{,}) + \cos\alpha_{,}\,(\sin\alpha\sin\beta - \sin\alpha_{,,}\sin\beta_{,,}) + \cos\alpha_{,,}\,(\sin\alpha_{,}\sin\beta_{,} - \sin\alpha\sin\beta)}{\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,} - \beta_{,,}} + \sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,} - \beta} + \sin\alpha\sin\alpha_{,}\sin\overline{\beta - \beta_{,}}}\right]\cot\lambda \quad (10.)$$

Write these, for the present, in the following form,

$$\sin\kappa = \frac{n}{p}\cot\lambda, \text{ and } \cos\kappa = -\frac{m}{p}\cot\lambda.$$

Then, since $\sin^2\kappa + \cos^2\kappa = 1$, we have

$$\tan^2\lambda = \frac{m^2 + n^2}{p^2}, \text{ and hence}$$

$$\left.\begin{aligned} \sin\lambda &= \frac{\sqrt{m^2 + n^2}}{\sqrt{m^2 + n^2 + p^2}} \\ \cos\lambda &= \frac{p}{\sqrt{m^2 + n^2 + p^2}} \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (11.)$$

Hence also we have

$$\left.\begin{aligned} \sin\kappa &= \frac{n}{p}\cot\lambda = \frac{n}{\sqrt{m^2 + n^2}}, \text{ and} \\ \cos\kappa &= -\frac{m}{p}\cot\lambda = \frac{-m}{\sqrt{m^2 + n^2}} \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12.)$$

Inserting these several values of the functions of κ and λ in either of the equations (2, 3, 4), we should get values of $\cos\rho$; but in these cases, the

symmetry of the result would be destroyed, as we should find the co-ordinates of one of the points involved differently from those of the other points. But by making the substitutions in *all*, adding the results, and taking one-third of the sum, we shall have a value of $\cos\rho$ in the proper form. The work, which is long, but involving no other difficulty, is suppressed here, and the result merely set down. That is

$$\cos\rho = \frac{\cos\alpha\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,}-\beta_{,,}} + \cos\alpha_{,}\sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,}-\beta} + \cos\alpha_{,,}\sin\alpha\sin\alpha_{,}\sin\overline{\beta-\beta_{,}}}{\sqrt{m^2+n^2+p^2}} \ldots (13.)$$

Making these several substitutions in (1), we have a complete solution of the problem. The denominator $\sqrt{m^2+n^2+p^2}$, appearing in all the terms of the expression, is omitted altogether.

$$\cos\alpha\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,}-\beta_{,,}} + \cos\alpha_{,}\sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,}-\beta} + \cos\alpha_{,,}\sin\alpha\sin\alpha_{,}\sin\overline{\beta-\beta_{,}} =$$

$$\cos\phi\,\{\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,}-\beta_{,,}} + \sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,}-\beta} + \sin\alpha\sin\alpha_{,}\sin\overline{\theta-\beta_{,}}\} -$$

$$\sin\phi\left\{\begin{array}{l}\cos\theta\{\cos\alpha(\sin\alpha_{,,}\sin\beta_{,,}-\sin\alpha_{,}\sin\beta_{,})+\cos\alpha_{,}(\sin\alpha\sin\beta-\sin\alpha_{,}\sin\beta_{,})+\cos\alpha_{,,}(\sin\alpha_{,}\sin\beta_{,}-\sin\alpha\sin\beta)\\ -\sin\theta\{\cos\alpha(\sin\alpha_{,,}\cos\beta_{,,}-\sin\alpha_{,}\sin\beta_{,})+\cos\alpha_{,}(\sin\alpha\sin\beta-\sin\alpha_{,}\sin\beta_{,})+\cos\alpha_{,,}(\sin\alpha_{,}\sin\beta_{,}-\sin\alpha\sin\beta)\end{array}\right\} \ldots (14.)$$

Cor. The condition that must subsist amongst the co-ordinates of three points on the sphere, that they may range in the same great circle, is,

$$\cos\alpha\sin\alpha_{,}\sin\alpha_{,,}\sin\overline{\beta_{,}-\beta_{,,}} + \cos\alpha_{,}\sin\alpha\sin\alpha_{,,}\sin\overline{\beta_{,,}-\beta} + \cos\alpha_{,,}\sin\alpha\sin\alpha_{,}\sin\overline{\beta-\beta_{,}} = 0. \ldots (15.)$$

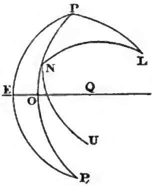
XIV.

To change the Origin and Direction of the Co-ordinates of a Spherical Curve.

1. Let the origin remain the same, but the prime meridian be changed for one whose longitude is $\beta_{,}$; then the transformation will be effected by writing $\theta + \beta_{,}$ instead of θ.

2. Let the origin be transposed to a point in the prime meridian, whose radius vector is a.

That is, let L be a point whose co-ordinates are to be changed from origin P and prime meridian PO, to another origin N in that meridian, and prime meridian NP.



Here $NP = \alpha$; $PL = \phi$; $PLN = \theta$; $PNL = \theta'$; and $NL = \phi'$. Then,

$$\cos\phi = \cos\phi' \cos\alpha + \sin\phi' \sin\alpha \cos\theta' \quad \ldots\ldots \quad (1.)$$
$$\sin\phi \sin\theta = \sin\phi' \sin\theta' \quad \ldots\ldots \quad (2.)$$

which conditions will enable us to express ϕ, θ as functions of ϕ', θ'.

From (1.) we find

$$\sin\phi = \pm\sqrt{1-(\cos\phi' \cos\alpha + \sin\phi' \sin\alpha \cos\theta')^2} \quad \ldots\ldots \quad (3.)$$

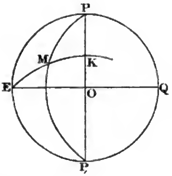
And from (2.) and (3.) we get readily

$$\sin\theta = \pm\frac{\sin\phi' \sin\theta'}{\sqrt{1-(\cos\phi' \cos\alpha + \sin\phi' \sin\alpha \cos\theta')^2}} \quad \ldots\ldots \quad (4.)$$

$$\cos\theta = \pm\frac{\cos\phi' \sin\alpha - \sin\phi' \sin\alpha \sin\theta'}{\sqrt{1-(\cos\phi' \cos\alpha + \sin\phi' \sin\alpha \cos\theta')^2}} \quad \ldots\ldots \quad (5.)$$

Which values of $\sin\phi$, $\cos\phi$, $\sin\theta$, and $\cos\theta$, will transform any curve referred to P and PO into one referred to N and NP.

3. When we wish to transform the origin from P to any point in the equator, as E, we have only to refer the locus to the meridian PE and origin P; and the formulæ (1, 3, 4, 5) become in reference to this,



$$\cos\phi = \sin\phi, \cos\theta'$$

$$\sin\phi = \pm\sqrt{1 - \sin^2\phi' \cos^2\theta'}$$

$$\cos\theta = \frac{\pm\cos\phi'}{\sqrt{1 - \sin^2\phi' \cos^2\theta'}}$$

$$\sin\theta = \frac{\pm\sin\phi' \sin\theta'}{\sqrt{1 - \sin^2\phi' \cos^2\theta'}}$$

4. If, besides changing the origin to N, it be required to change the direction also of the prime meridian to NU, which makes with PN an angle PNU $= \mu$, then we have only to write in equation (5.) for θ' the angle $\theta' + \mu$, and the transformation will be completed.

5. We may particularly specify the case where the transformation is to the opposite pole of the equator,—a case of frequent occurrence in the investigation of different theorems, and still more frequently in the examination of the course of the curve whose equation we may desire to examine.

Here $a = \pi$, and $\mu = 0$, $\theta' = \theta$. Hence,

$$\sin\phi = \sin\phi'; \quad \cos\phi = -\cos\phi'; \quad \tan\phi = -\tan\phi' : \text{ and}$$
$$\sin\theta = \sin\theta'; \quad \cos\theta = \cos\theta'; \quad \tan\theta = \tan\theta'$$

&c. &c. &c.

Schol. 1. We may illustrate this transformation in the case of the circle. Let its equation be

$$\cot\phi = -\tan\lambda \cos(\theta - \kappa)$$

$$\text{or, } \frac{\cos\phi}{\sin\phi} = -\frac{\sin\lambda}{\cos\lambda}(\cos\theta\cos\kappa + \sin\theta\sin\kappa) \quad \ldots\ldots \quad (6.)$$

Inserting the values of the functions of ϕ and θ from (2, 3, 4, 5) in (6), we have, dropping the common denominator $\sqrt{1-(\cos\alpha\cos\phi' + \sin\alpha\sin\phi'\cos\theta')^2}$, the following result, as the transformed equation which we are in search of,

$$(\sin\lambda\cos\alpha + \cos\lambda\cos\kappa\sin\alpha)\cot\phi' = -\{(\sin\lambda\sin\alpha - \cos\lambda\cos\alpha\cos\kappa)\cos\theta' + \sin\kappa\cos\lambda\sin\theta'\} \ldots (7.)$$

If we put $\kappa' = \tan^{-1}\dfrac{\sin\kappa\cos\lambda}{\sin\lambda\sin\alpha - \cos\lambda\cos\alpha\cos\kappa}$, and

$\lambda' = \cot^{-1}(\cos\alpha\sin\lambda + \cos\lambda\sin\alpha\cos\kappa)$, then we shall have (7.) transformed into

$$\cot\phi' = -\tan\lambda'\cos(\theta' - \kappa') \quad \ldots\ldots\ldots\ldots\ldots \quad (8.)$$

the same general form as before.

Schol. 2. Many interesting inquiries respecting transformations of co-ordinates must be delayed for the present, as the room which I can here allot to the inquiry is already filled. One principle to be established is, that no transformation changes the *order* of a curve on the sphere, more than on a plane, or in space of three dimensions. Much as I wish to enter upon this matter here, I am compelled to waive it, on account of the preliminary discussion of what determines the order of a spherical curve, and what test will decide it.

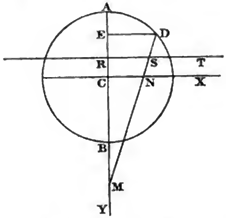
XV.

The great variety of cases in which we may have occasion to examine the projection of a spherical curve upon a plane, renders it necessary to lay down a general formula of projection for this purpose. This will be done in the following simple *lemmas*.

1. A point D on the surface of the sphere is projected from a given point in the axis of the sphere upon a plane parallel to the equator. Find the distance of the projection from the centre* of the plane.

* "Centre of the plane" is used to signify the point where the plane of projection is cut by the axis of the sphere.

Let M be the projecting point, A, B the poles, C the centre of the sphere, ADB the meridian in which the point is situated, and RST its intersection with the plane of projection. Then S is the point into which D is projected, and RS is the line or distance sought.



Put $CM = b$; $BC = a$; $CR = c$; $AD = \phi$; $RS = v_{,}$; and $CN = v$. Then,

$$EM = b + a \cos\phi, \text{ and}$$

$$EM : MC :: ED : CN, \text{ or}$$

$$b + a \cos\phi : b :: a \sin\phi : v = \frac{ab \sin\phi}{b + a \cos\phi} \quad \ldots\ldots\ldots\ldots \quad (1.)$$

$$\text{Again, } CM : RM :: CN : RS, \text{ or}$$

$$b : b + c :: v : v_{,} = \frac{(b+c)v}{b} = \frac{a(b+c)\sin\phi}{b + a\cos\phi} \quad \ldots\ldots\ldots \quad (2.)$$

which is the distance sought.

2. The angles made by the meridians $\widehat{AD}$, &c. are obviously equal to their projections on the plane RS.

3. Dropping the subscribed accent from $v_{,}$ in (2), and resolving the equation for $\sin\phi$ and $\cos\phi$, we obtain

$$\sin\phi = -\frac{b(b+c)v \pm v\sqrt{(a^2-b^2)v^2 + a^2(b+c)^2}}{a\{v^2 + (b+c)^2\}} \quad \ldots\ldots\ldots \quad (3.)$$

$$\cos\phi = -\frac{bv^2 \pm (b+c)\sqrt{(a^2-b^2)v^2 + a^2(b^2+c)^2}}{a\{v^2 + (b+c)^2\}} \quad \ldots\ldots\ldots \quad (4.)$$

4. By means of (3) and (4) we can assign the polar equation the projection of any spherical curve upon a plane perpendicular to the axis, in which the projecting point is situated. Let us take, for instance, the three common projections, the *orthographic*, *stereographic*, and *gnomonic*.

The *Orthographic*. Here $b =$ infinite, and the equations are,

$$\left.\begin{array}{c} v = a\sin\phi, \text{ or} \\ \sin\phi = \dfrac{v}{a};\ \cos\phi = \pm\dfrac{\sqrt{a^2 - v^2}}{a}, \ \&\text{c.} \end{array}\right\} \dots\dots\dots \quad (5.)$$

The *Stereographic*. Here $b = a$, and $c = 0$; and

$$v = \frac{a\sin\phi}{1+\cos\phi}; \text{ from which}$$

$$\cos\phi = \frac{\pm a^2 - v^2}{a^2 + v^2} \quad \dots\dots\dots\dots\dots\dots\dots \quad (6.)$$

Insert this in $\cos^2\phi + \sin^2\phi = 1$, and we get

$$\left.\begin{array}{l} \sin\phi = \pm\dfrac{2av}{a^2 - v^2} \\ ^*\sin\phi = \pm\dfrac{0}{a^2 - v^2} = 0 \end{array}\right\} \dots\dots\dots\dots\dots\dots\dots \quad (7.)$$

The *Gnomonic*. Here $b = 0$, and $c = a$; and the equation becomes

$$v = \frac{a\sin\phi}{\cos\phi} = a\tan\phi$$

$$\therefore\ \tan\phi = \frac{v}{a};\ \cos\phi = \frac{\pm a}{\sqrt{v^2 + a^2}};\ \text{and } \sin\phi = \frac{\pm v}{\sqrt{v^2 + a^2}}. \ \dots \quad (8.)$$

5. The different "globular projections" that have been proposed for the construction of geographical maps, by taking as projecting points different

* These two equations are foreign to the inquiry. The manner of their appearance here is easily shewn; but the extent to which this paper runs, forbids my enlarging upon this topic at present.

points in AB, are represented by equations (3.), (4.) Those, for instance, of Lahire, Arrowsmith, Nicholson, &c.; but as there is nothing particularly interesting, in a mathematical point of view, attached to these inquiries, we shall pass them over. We may just state, however, that when we wish to assign the *cone* whose intersection with the sphere is any specific spherical curve, and whose centre is at the centre of the sphere, the same equations apply as are employed for the interchange of rectangular and polar co-ordinates in the usual processes of analytical geometry.

$$\left.\cos\theta = \frac{\pm x}{\sqrt{x^2+y^2}}; \quad \sin\theta = \frac{\pm y}{\sqrt{x^2+y^2}}\right\} \ldots\ldots\ldots\ldots\ldots\ldots (9.)$$

$$\left.\cos\phi = \frac{\pm z}{\sqrt{x^2+y^2+z^2}}; \quad \sin\phi = \pm\sqrt{\frac{x^2+y^2}{x^2+y^2+z^2}}\right\} \ldots\ldots (10.)$$

The equations (9.) also serve to transform (5, 6, 7, 8.) into rectangular curves. By means of (9.) (10.) it will be remarked, that the spherical equations of the hectemoria were converted into rectangular ones.—(See art. XXI. of that paper.)

I shall not dwell longer on this part of the subject, which, on account of its very elementary character, I would gladly have omitted, but that some of its results are necessary in our future investigations.

XVI.

1. The length of any curve on the sphere, referred to the co-ordinates ϕ and θ, is

$$L = \int\sqrt{d\theta \,.\, \sin\phi + d\phi^2} + C.$$

For, let MS be an element of the curve lying between consecutive meridians PE, PQ, and let EQ be a corresponding element of the equator. Then,

$$MN^2 + SN^2 = MS^2; \text{ that is, } MS^2 = EQ^2 \sin^2\phi + SN^2;$$
$$= \sin^2\phi\, d\theta^2 + d\phi, \ldots\ldots\ldots (1.)$$

2. The inclination of an element of a curve to the meridian, is obtained by the same considerations, viz.

$$\tan \text{MSN} = \frac{\text{MN}}{\text{SN}} = \frac{d\theta \sin \phi}{d\phi}, \ldots \ldots \ldots \ldots \ldots (2.)$$

3. The area of a spherical curve is obtained by integrating

$$\text{A} = \int (1 - \cos \phi)\, d\theta + \text{C}$$

Considering the elementary triangle whose sides are ϕ, $\phi + d\phi$, and included angle $d\theta$, we have its area, by the usual rule for the spherical excess,

$$\tan \tfrac{1}{2} \text{D} = \frac{\sin \frac{\phi}{2} \sin \frac{\phi + d\phi}{2} \sin d\theta}{\cos \left(\phi + \frac{d\phi}{2}\right) + 2 \sin \frac{\phi}{2} \sin \frac{\phi + d\phi}{2} \cos^2 \frac{d\theta}{2}}, \ldots \ldots \ldots (3.)$$

But, $$\sin d\theta = d\theta; \ \cos \frac{d\theta}{2} = 1;$$

$$\cos \left(\phi + \frac{d\theta}{2}\right) = \cos \phi \cos \frac{d\phi}{2} - \sin \phi \sin \frac{d\phi}{2}$$

$$\sin \left(\frac{\phi + d\phi}{2}\right) = \sin \frac{\phi}{2} \cos \frac{d\phi}{2} + \cos \frac{\phi}{2} \sin \frac{d\phi}{2}.$$

Whence, $$\cos \left(\phi + \frac{d\phi}{2}\right) = \cos \phi - \tfrac{1}{2} \sin \phi d\phi,$$

$$\sin \frac{\phi + d\phi}{2} = \sin \frac{\phi}{2} + \tfrac{1}{2} \cos \frac{\phi}{2} d\theta.$$

Making these substitutions, we find (1.) converted into

$$\tan \tfrac{1}{2} \text{D} = \frac{\sin \frac{\phi}{2} \left(\sin \frac{\phi}{2} + \frac{1}{2} d\phi \cos \frac{\phi}{2}\right) d\theta}{(\cos \phi - \frac{1}{2} \sin \phi d\phi) + 2 \sin \frac{\phi}{2} (\sin \frac{\phi}{2} + \frac{1}{2} \cos \frac{\phi}{2} d\phi)}, \ldots \ldots \ldots (4.)$$

The infinitesimal addends of this being cancelled, the expression becomes

$$\tan \tfrac{1}{2} \text{D} = \frac{\sin^2 \frac{\phi}{2} d\theta}{\cos \phi + 2 \sin^2 \frac{\phi}{2}} = \tfrac{1}{2} (1 - \cos \phi)\, d\theta, \ldots \ldots \ldots \ldots \ldots \ldots (5.)$$

But $\tan \frac{1}{2} D = \frac{1}{2} dA$, in the case of elementary triangles, and hence we have ultimately

$$A = \int (1 - \cos \phi)\, d\theta + \text{const.}\ *$$

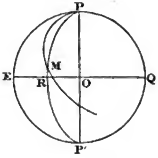
XVII.

THE EQUABLE SPHERICAL SPIRAL.

We shall now proceed to the application of these principles to spherical loci, different from those (viz. the circles) which we have been hitherto considering. It would be easy to imagine different methods by which curves may be traced as well on the sphere as *in plano;* but it will perhaps be more agreeable, and at the same time better display the advantages of the method here employed, if we examine sphericals already imagined, and which have been often treated by other methods. We shall therefore commence by considering the Spiral of Pappus †. We may state it more generally thus:

A meridian PRP′ *revolves about the axis* PP′ *of a sphere, whilst a point* M *in it moves from* P *in the direction of* PRP′; *these motions being uniform, and in a given ratio. What is the locus of* M?

Fig. 8.



* Certain precautions, which are not very prominently brought forward *in plano*, are necessary in the use of this and of all other forms of spherical equations. We shall explain at a future time.

† Pappus, Coll. Math. lib. iv. prop. 30.

Reckoning the angle θ to commence at PÉP′, and the radius-vector PM being denoted by ϕ, and calling $m:n$ the given ratio, we have

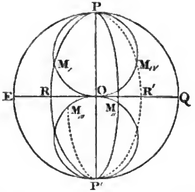
$$m\phi = n\theta \ldots\ldots\ldots\ldots\ldots\ldots\ldots(1.)$$

We may examine a few of the cases that arise from giving particular values to the constants m and n.

1. Let $m = n$, then $\phi = \theta$.

Here we shall have a curve somewhat resembling the lemniscata of Bernoulli in its general appearance, and lying wholly upon *one* hemisphere.

Fig. 9.



During the first quadrant of longitude*, the point M will be in the spherical octant EOP; and when one quadrant of longitude has been described, it will be found at O.

During the second quadrant, the point ($M_{,}$) will be found in the octant OP′Q; and at π of longitude, the point will merge in P′.

* In these figures, we suppose the sphere orthographically projected on the plane of the meridian PEP′Q. As this will require us to represent both the hemispheres on one plane, we shall distinguish that which is between us and the meridian by the name of *convex* and the other by the name *concave* hemisphere. We shall trace in *full line* the parts of the locus that lie on the convex hemisphere, and in *dots* those which lie on the concave. The same letters are used for corresponding points on both hemispheres; but those belonging to the concave are accentuated.

When the longitude is greater than π, the polar distance will be reckoned on a dotted meridian PR′P′M, which comes up (on account of its also being greater than π) on the convex side in $P'M_{,,,}$; and it therefore lies in EP′O so long as

$$\frac{3\pi}{2} > \theta > \pi.$$

And it arrives again at O, when ϕ and θ have attained each the value of three quadrants.

During the interval between the values of $\frac{3\pi}{2}$ and 2π taken by ϕ and θ, the point will be in the octant POQ, and at the termination of that period, it will have returned to P, the origin. It is also evident, that, by continuing the process, the same succession of changes will take place, and the same path on the surface of the sphere traced out. The figure whose orthographic projection is $PM, OM_{,,}\ P'M_{,,,}\ OM_{\text{iv}}$ is the *complete locus*, therefore, of the equation

$$\phi = \theta.$$

2. Let $m = 2n$, or $\phi = \frac{1}{2}\theta$: then the figure (10.) is the orthographic

Fig. 10.

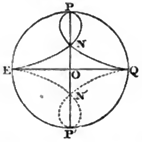


Fig. 11.

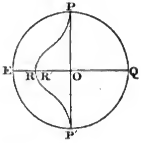
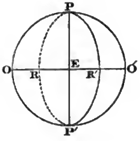


Fig. 12.

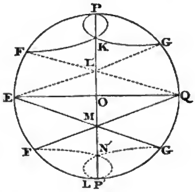


representation; and if $m = \frac{1}{2}n$, or $\phi = 2\theta$, the figure (11.) is the result, In this there is to be understood a dotted branch lying under the branch PRP′. We have therefore also traced in Fig. 12. the two branches visible, by taking POP′O′ as the convex hemisphere, instead of PEP′Q.

3. Let $\phi = \frac{\theta}{4}$, which is the particular case considered by Pappus. Here the curve cuts the equator after a complete revolution of θ. A second revo-

lution of θ carries the point to the opposite pole L. A third brings it back to the equator at Q; and a fourth back to P. The general figure of the curve will be well understood from Fig. 13., keeping in mind that all cuspi-

Fig. 13.



dated points in the circle PELQ, as F, G, H, I are indicative of the dotted and traced branches whose projections meet in those points, forming continuous portions of the curve.

4. If m and n be commensurable, the branches of the spirals traced out will return in the same order, and each coalesce with its corresponding one, after θ has attained the value $2mn\pi$; but if they be incommensurable, there can never be any reduplication of the spirals traced out.

XVIII.

The rectification of these spirals can always be effected by arcs of an ellipse, but never by any simpler functions,

For, in this case, we have $\theta = \frac{m}{n}\phi$, and $d\theta = \frac{m}{n}d\phi$. Hence, the element of the arc becomes

$$dA = \sqrt{1 + \frac{m^2}{n^2}\sin^2\phi \cdot \frac{m}{n}} \cdot d\theta = \mp\sqrt{\frac{m^2+n^2}{n^2}}\sqrt{1 - \frac{m^2}{m^2+n^2}\sin^2\left(\frac{\pi}{2}-\phi\right)}\, d\left(\frac{\pi}{2}-\phi\right)$$

1. When $\phi = \theta$, we have

$$d\text{A} = \mp \sqrt{2} \,.\, \sqrt{1 - \tfrac{1}{2} \sin^2 \left(\frac{\pi}{2} - \phi\right)}\, d\left(\frac{\phi}{2} - \phi\right).$$

2. When $\phi = \frac{\theta}{4}$, we have $\frac{m}{n} = 4$, or $m = 4$ and $n = 1$. Hence, the length of the spiral of Pappus is

$$= \mp \sqrt{17} \int \sqrt{1 - \frac{1}{17} \sin^2 \left(\frac{\pi}{n} - \phi\right)} \,.\, 4d\phi + \text{const.}$$

It is unnecessary to pursue these rectifications farther, as the character of the inquiry is well known to geometers.

XIX.

The areas of these spirals can in some cases be expressed by means of spherical lunes.

The general expression for the element of the arc is

$$d\text{A} = (1 - \cos \phi) \frac{m}{n} d\phi \quad \ldots\ldots (1.)$$

$$\text{or A} = \frac{m}{n} (\phi - \sin \phi) + \text{const.} \quad \ldots\ldots (2)$$

1. Let $m = n$. Then taking the integral between 0 and $\frac{\pi}{2}$, we have for one-fourth of the area of the curve,

$$\text{A} = \frac{\pi}{2} - 1,$$

or for the whole area, A $2 = \pi - 4$. The radius in all these cases being unity.

The residue of the hemisphere is

$$2\pi - (2\pi - 4) = 4,$$

which being equal to the square of the diameter of the sphere, that residue is quadruple.

2. This, indeed, is one of the solutions given by James Bernoulli to the celebrated Enigma of Viviani proposed to the Analysts of his day, in 1682. Nor does our construction differ in any particular worthy of mention from that given by the illustrious Professor of Basle *. The construction of Viviani himself, which he published without demonstration, and which has usually (by writers implicitly following the statements of Grandi and Montucla) been characterised as the most elegant given of that problem. But it will be presently shown *that the methods of* Viviani *and* James Bernoulli *are identical* †. I shall here, however, remark, that the second and third solutions of Bernoulli are inaccurate, as professing to leave quadrable spaces on the surface of the sphere. The second of these is given by the condition

$$m \sin \phi = n \sin \theta \ \ddagger,$$

and the elementary area is,

$$d\,A = (1 - \cos \phi)\, d \sin^{-1} \frac{m}{n} \sin \phi$$

$$= \frac{m}{n}(1 - \cos\phi) \cdot \frac{\cos \phi \, d\, \phi}{\sqrt{1 - \frac{m^2}{n^2} \sin^2 \phi}} \quad \ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

which a little management will transform into *elliptic*, but not into *circular* functions. Hence the problem does not receive a solution from that process.

It is easy to discover the cause of this oversight of Bernoulli, and his commentator. They forgot that it was a residue that was to answer the question. Had it been the area of the figure itself that was quadrable, then, of course, any other which had a rational ratio to it would have been quadrable too: but as the area of the figure was required to be a

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* See his Works, collected by Cramer, vol. ii. p. 512.

† See Art. XX. Indeed this has been done by Bernoulli himself, very simply and elegantly, Op. tom. ii. p. 744.

‡ Bern. Op. ii. p. 513.

certain assignable portion of a sphere, minus an assignable square, this method not giving that result, does not apply to the case in question.

The same remark may be made respecting his third solution, or that which is expressed by the general form of equation (2.). If we take $\frac{m}{n}\phi$ equal to the requisite spherical lune, then $\frac{m}{n}\sin\phi$ will be irrational and transcendental. So that, in this case neither the curvilinear area nor the residue of the spherical surface will fulfil the condition. And if we take $\phi = \pi$, 2π, &c., we still have $\frac{m}{n}\pi$, $\frac{m}{n} . 2\pi$, which will not fulfil the conditions neither. We see, then, that this method also fails, except in particular relations amongst the constants m and n.

3. When $\phi = \frac{\theta}{4}$, we have the spiral of Pappus, that is the curve by which he abstracted such a portion of a sphere as was perfectly quadrable. Here

$$d\,A = (1 - \cos\phi)\, 4\, d\,\phi, \text{ or}$$

$$A = 4\,\phi - 4\sin\phi + \text{const.} \qquad (4.)$$

Now, when $\phi = \frac{\pi}{2}$, we have $A = 2\pi - 4$,

which, subtracted from 2π, leaves, as before, for a residue of the hemisphere, the square of the diameter of the sphere. This agrees with the usual solution of the problem.

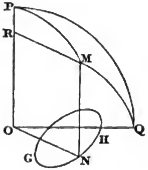
4. Let $\phi = \frac{\theta}{2}$: then $A = 2\phi - 2\sin\phi + C$. This taken between 0 and π gives $A = 2\pi$, or the spiral bisects the superficies of the sphere.

5. Take $\phi = \frac{1}{8}\theta$: then $A = 8\phi - 8\sin\phi$; and this taken from 0 to to $\frac{\pi}{2}$ gives $4\pi - 8$; or the residue of the whole sphere equal to eight times the square of radius. In like manner, several of the values of $\frac{m}{n}$ may be found, which, when ϕ is taken between certain limits, the residue of the sphere, or of particular portions of it, shall be quadrable.

XX.

The identity of the solutions of Viviani and Bernoulli of the Florentine Enigma, was asserted in (XIX, 2): we shall prove it here.

Fig. 14.



Let a plane perpendicular to the axis cut the cylinder in the curve NGH; and let a diametrical plane, perpendicular to the plane NGH, cut the sphere in PQ, and the plane NGH in OQ. Take the pole of NGH for origin of ϕ; and let the equation of the generating curve NGH (referred to pole O, and polar angle QON) be $f(\theta, v)$. Draw MN $\perp$ to the plane NHG, then MN is one of the edges of the cylinder, and M a point in the curve of penetration. Draw MR parallel to NO.

$$\text{Then RM} = \text{ON} = v = a \sin\phi = \sin\phi \text{ (rad.} = 1\text{)}.$$

Hence the equation of the curve of penetration of the sphere and *any* cylinder is

$$f(\theta, \sin\phi) = 0\,^{*} \ldots\ldots\ldots\ldots\ldots\ldots (1.)$$

But in the *circle* on OQ, to which the present question refers, we have $v = a\cos\theta = \cos\theta$ to rad. 1. Insert this in (1).

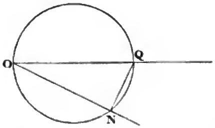
Hence the equation of the curve becomes, (see fig. at top of next page),

$$\sin\phi = \theta.$$

$$\theta + \phi = \frac{\pi}{2} \ldots\ldots\ldots\ldots\ldots\ldots (2.)$$

* For MPQ = NOQ = θ; or θ is the same both on the spherical equator and its projection on the plane MHG. It may also be remarked, that there is no essential difference between Bernoulli's own proof and the one above given, except the notation.

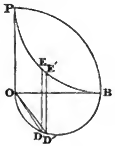
FIG. 15.



This is identical with BERNOULLI's solution, and differs from that given above only in having a different origin of θ.

XXI.

FIG. 16.



It may be remarked in passing, that the other known properties of this section, given by BOSSUT*, MONTUCLA†, and IVORY‡, may be easily obtained by methods analogous to those above employed. To take as an instance, that of MONTUCLA, viz. that the surface of the intercepted portion of the cylinder itself is quadrable. Take any corresponding points D and E in the base of the cylinder and the curve of penetration. Then

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* Mém. de l'Institut. tom. ii. p. 228.

† Hist. des Math. tom. ii. p. 94.

‡ LEYBOURN's Mathematical Repos. vol. i. pt. ii. p. 1. New Series. See also WOODHOUSE in Phil. Trans. 1801, p. 153. Other works are referred to by historical writers: but as additional detail would be inconsistent with my plan, I shall not consider further examination to be at present necessary.

ED $= \cos\phi = \sin\theta$, and the element of the cylindrical surface is $2 \sin\theta\, d\theta$: or integrating between the limits 0 and $\frac{\pi}{2}$, we have the semicylinder $= 2$, and the whole cylinder above and below the equator $= 8$.

XXII.

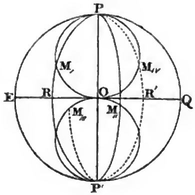
We mentioned, that in the use of the formula $A = \int (1 - \cos\phi)\, d\theta$, certain precautions are necessary to be observed. We now proceed to explain them: to do which we shall commence by an example. Resuming the equation (XX, 2), or

$$A = \frac{m}{n}(\phi - \sin\phi) + \text{const.}$$

And take $m = n$. Hence $A = \phi - \sin\phi + \text{const.}$................(1.)

The area here signified is the portion of the surface over which the radius vector PM passes on the surface of the sphere. If, therefore, we seek the area within the limits of $\phi = 0$ and $\phi = \frac{\pi}{2}$, we shall have no space passed over by the radius vector but what lies *within* the curve in question. If we take a greater value of ϕ, we shall have a portion of spherical surface traced by the radius vector which lies *without* the curve; in short, the ***residue*** of the hemisphere after the enclosure of branch $OM_{\prime\prime}L$ has been removed. As we proceed onward to $\pi \angle \phi \angle \frac{3\pi}{2}$, we find the half of the

FIG. 9.



concave hemisphere P′Q′L′, as well as the area of the branch $LM_{\prime\prime}O$ described. And finally, during the remaining quadrant of increase of ϕ, the remaining half P′E′L of the concave hemisphere, as well as the portion LOPQL will be passed over by the radius vector. Adding these together, we have

$$PM_{\prime}O + POM_{\prime\prime}LQP + P'Q'L' + LM_{\prime\prime}O + P'L'E' + LOPQL, \text{ or}$$

$$(PM_{\prime}O + LOPQL) + (POM_{\prime\prime}LQP + LM_{\prime\prime}O) + P'Q'L' + P'L'E'.$$

These make up four spherical quadrantal lines, or the whole surface of the sphere: and such would have been the anticipated result of taking ϕ between limits 0 and 2π. *But this is not the case:* for then we have only

$$A = 2\pi,$$

or to a single hemisphere. How is this to be explained? I have formed to myself a theory of the apparent anomaly, but it is connected with so many considerations, that the discussion would take up more room than I can allow myself in the present paper*. Whilst, therefore, I defer this discussion till a future time, I think it necessary to state, that the difficulty may always be evaded by transposing the origin of co-ordinates to the nodal point of the curve, as, for instance, to the point O in the preceding figures. The formulæ of transformation adapted to this case are given in (XIV. 3), and the process is too simple to need farther illustration.

If, however, the figure be composed (as in the present case) of four equal branches, it will be sufficient to find the area of one, and take its quadruple for the whole area. This is analogous to those processes in plane curves, where we find the areas of the branches above and below the axis of x separately, and take (in symmetrical curves) double of one of them for the whole area.

* Some preliminary considerations intimately connected with this subject, are given in Note (B) at the end.

XXIII.

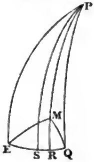
LEXELL'S THEOREM *.

On the surface of the sphere, the line in which are situated the vertices of all the triangles, having the same base and the same surface,—is a less circle of the sphere.

By resolving (III. 3.) supposing $\alpha_{\prime\prime\prime}\ \beta_{\prime\prime\prime}$ the variable co-ordinates of the point in question, and the other quantities engaged in the expression (viz. $\alpha_{\prime}\ \beta_{\prime}$, $\alpha_{\prime\prime}\ \beta_{\prime\prime}$ and $\cos \frac{E}{2}$) as constants, we shall find an equation to the circle: but as this reduction is somewhat operose, and the resulting expression rather complex, we shall take a shorter course, as follows:

Let EQ, the base, be bisected in S, and EP, SP, QP, drawn to the

FIG. 17.



pole of EQ. Take P and PS as origin of ϕ and θ. Put $ES = \gamma$, $SR = \theta$, and $PM = \phi$. Puta lso, for the present, $\chi = \frac{\pi}{2} - \phi = MR$. Let the spherical excesses of the triangles EMR, RMQ, and EMQ be respectively ϵ, $\epsilon_{\prime}$ and E. Then, since the angles at R are right, we have

---

* Acta Petrop. tom. v.—Dr BREWSTER's Translation of LEGENDRE's Geometry, p. 266, and other places.

$$\left.\begin{aligned}\cot\frac{\epsilon}{2} &= \cot\frac{\gamma+\theta}{2}\cot\frac{\chi}{2}\\ \cot\frac{\epsilon_{\prime}}{2} &= \cot\frac{\gamma-\theta}{2}\cot\frac{\chi}{2}\end{aligned}\right\}\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1.)$$

From (1.) we get, by taking $\cot^{-1}$ of each side of each equation, and adding the results,

$$\frac{E}{2} = \cot^{-1}\cot\frac{\chi}{2}\cot\frac{\gamma+\theta}{2} + \cot^{-1}\cot\frac{\chi}{2}\cot\frac{\gamma-\theta}{2}, \ldots\ldots\ldots\ldots (2.)$$

or, taking cotan of each side, it is, after slight reduction,

$$\cot\frac{E}{2} = \frac{\cos^2\frac{\chi}{2}\cos\frac{\gamma+\theta}{2}\cos\frac{\gamma-\theta}{2} - \sin^2\frac{\chi}{2}\sin\frac{\gamma+\theta}{2}\sin\frac{\gamma-\theta}{2}}{\cos\frac{\chi}{2}\sin\frac{\chi}{2}\left\{\cos\frac{\gamma+\theta}{2}\sin\frac{\gamma-\theta}{2} + \cos\frac{\gamma-\theta}{2}\sin\frac{\gamma+\theta}{2}\right\}} =$$

$$= \frac{\cos^2\frac{\chi}{2}(\cos\theta+\cos\gamma) - \sin^2\frac{\chi}{2}(\cos\theta-\cos\gamma)}{\sin\chi\sin\gamma} =$$

$$= \frac{\cos\theta\cos\chi+\cos\gamma}{\sin\chi\sin\gamma}. \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

Or, reducing (3.), and putting $\frac{\pi}{2}-\phi$ for χ, it becomes ultimately

$$\cos\gamma\sin\frac{E}{2} = \cos\frac{E}{2}\sin\gamma\cos\phi - \sin\frac{E}{2}\sin\phi\cos\theta \ldots\ldots\ldots\ldots (4.)$$

This is the equation of a circle (Vid. I. 1.), the co-ordinates of whose centre and whose radius are found from these equations, viz.

$$\left.\begin{aligned}\kappa &= 0,\\ \tan\lambda &= -\tan\frac{E}{2}\operatorname{cosec}\gamma,\\ \cos\rho &= \frac{\cos\gamma\sin\frac{E}{2}}{\sqrt{1-\cos^2\frac{E}{2}\cos\gamma}},\end{aligned}\right\}\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (5.)$$

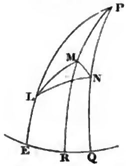
These results perfectly correspond with those obtained by LEGENDRE, as above referred to, when the difference of notation is attended to.

XXIV.

SPHERICAL ELLIPSE AND SPHERICAL HYPERBOLA.

The sum or difference of the arcs drawn from given points or foci on the sphere, to the different points of a sought curve, are given to find the equation of that curve.

Fig. 18.



Let $\alpha_{\prime}\beta_{\prime}$ and $\alpha_{\prime\prime}\beta_{\prime\prime}$ be the given points, $2i$ the said sum or difference, and $\phi\theta$ one of the current points of the curve. Then, these two distances are (by II.),

$$\left.\begin{array}{l}\delta_{\prime} = \cos^{-1}\{\cos\alpha_{\prime}\cos\phi + \sin\alpha_{\prime}\sin\phi\cos(\theta-\beta_{\prime})\} \\ \delta_{\prime\prime} = \cos^{-1}\{\cos\alpha_{\prime\prime}\cos\phi + \sin\alpha_{\prime\prime}\sin\phi\cos(\theta-\beta_{\prime\prime})\}\end{array}\right\}\ldots\ldots\ldots\ldots(1.)$$

The condition then becomes $\delta_{\prime} \pm \delta_{\prime\prime} = 2i$, and therefore,

$$\left.\begin{array}{l}2i = \cos^{-1}\{\cos\alpha_{\prime}\cos\phi + \sin\alpha_{\prime}\sin\phi\cos\overline{\theta-\beta_{\prime}}\} \pm \\ \quad\pm\cos^{-1}\{\cos\alpha_{\prime\prime}\cos\phi + \sin\alpha_{\prime\prime}\sin\phi\cos\overline{\theta-\beta_{\prime\prime}}\}\end{array}\right\}\ldots\ldots\ldots\ldots(2.)$$

Taking cosines of both sides, and reducing the results to their simplest form, we get

$$\cos^2\delta_{\prime} - 2\cos 2i\cos\delta_{\prime}\cos\delta_{\prime\prime} + \cos^2\delta_{\prime\prime} = \sin^2 2i\ldots\ldots\ldots\ldots(3.)$$

or, restoring the values of $\delta_{\prime}$ and $\delta_{\prime\prime}$ given by (1.) and reducing the results, we have

$$\left.\begin{array}{l}\sin^2 2i = \cos^2\phi\{\cos^2\alpha_{\prime} - 2\cos 2i\cos\alpha_{\prime}\cos\alpha_{\prime\prime} + \cos^2\alpha_{\prime\prime}\} + 2\cos\phi\sin\phi\,\times \\ \quad\times\{\sin\alpha_{\prime}(\cos\alpha_{\prime} - \cos\alpha_{\prime\prime}\cos 2i)\cos\overline{\theta-\beta_{\prime}} + \sin\alpha_{\prime\prime}(\cos\alpha_{\prime\prime} - \cos\alpha_{\prime}\cos 2i)\cos\overline{\theta-\beta_{\prime\prime}}\} + \\ \quad+\sin^2\phi\{\sin^2\alpha_{\prime}\cos^2\overline{\theta-\beta_{\prime}} - 2\cos 2i\sin\alpha_{\prime}\sin\alpha_{\prime\prime}\cos\overline{\theta-\beta_{\prime}}\cos\overline{\theta-\beta_{\prime\prime}} + \sin^2\alpha_{\prime\prime}\cos^2\overline{\theta-\beta_{\prime\prime}}\}\end{array}\right\}\ldots(4.)$$

If we write it under the temporary form of

$$\sin^2 2i = \mathrm{A} \cos^2 \phi + 2\mathrm{B} \cos \phi \sin \phi + \mathrm{C} \sin^2 \phi,$$

we get

$$\sin \phi = \pm \sqrt{\frac{\overline{\mathrm{C} - \mathrm{A}} \sin^2 2i - \mathrm{B}^2 \pm \sqrt{\mathrm{B}^2 - 2(\mathrm{C} - \mathrm{A}) \sin^2 2i + \sin^4 2i}}{(\mathrm{C} - \mathrm{A})^2 + \mathrm{B}^2}}, \quad \text{.........(5.)}$$

This value of $\sin \phi$ is fourfold, composed of two equal pairs of values, the individuals of each pair being equal, and affected with opposite signs. This indicates pairs of points on the radius-vector, the individuals of each pair being at the distance of π from each other, that is, diametrically opposite; thus forming two equal and opposite branches, situated one on the convex and the other on the concave hemisphere. This is clear, if we reflect that $-\sin \phi = \sin (\sin \pi + \phi)$; and that hence whatever curve $\sin \phi$ traces out, the equation of $-\sin \phi$ will trace out a similar one upon the opposite hemisphere.

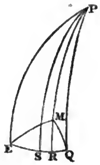
In the process of forming equation (2.) we lost all traces of the distinction of cases of the problem, whether $2i$ be equal to the sum or to the difference of the focal distances of the points of the curve whose general equation is (3.): but we find it re-appearing in the radical of equation (4.), for the + refers to the locus where the *sum*, and — where the *difference*, is given. When the *sum* is taken, the locus is composed of two isolated but equal and similar branches, situated point for point diametrically opposite to one another, as in Fig. 20. When the *difference* is taken, the locus is composed of four isolated branches, ranged in two pairs, the corresponding points in either pair being diametrically opposite, as in the former case. The discussion of the *general equation* of the spherical conic sections is full of interesting results, especially for the analogies which they bear to lines of the second degree, and the oftentimes curious modifications which that analogy undergoes. This we cannot, however, for want of room, enter upon in the present paper *.

The general equation (3.) is not, however, necessary when our object is simply to investigate the character of this pair of curves isolated from all others which may be traced on the same sphere. For this purpose, we may suppose the equator to pass through the foci, and the prime meridian to bi-

* See also Note (C) at the end.

sect· that distance. Then, we shall have $a_{,} = a_{,,} = \frac{\pi}{2}$; and $-\beta_{,} = +\beta_{,,}$ ($= \gamma$, suppose). Hence,

$$\sin a_{,} = \sin a_{,,} = 1$$
$$\cos a_{,} = \cos a_{,,} = 0$$
$$\sin \beta_{,} = -\sin \gamma$$
$$\sin \beta_{,,} = +\sin \gamma$$
$$\cos \beta_{,} = \cos \beta_{,,} = \cos \gamma.$$



These reduce (3.) to

$$\sin \phi = \pm \frac{\sin i \cos i}{\sqrt{\cos^2 \gamma \sin^2 i \cos^2 \theta + \sin^2 \gamma \cos^2 i \sin^2 \theta}} \quad \text{.............(6.)}$$

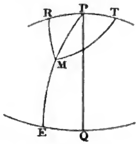
Suppose $\gamma = 0$, or the foci, to coalesce: then

$$\sin \phi = \frac{\pm \sin i \cos i}{\sin i \cos \theta} = \pm \frac{\cos i}{\cos \theta}, \quad \text{.................................(7.)}$$

the equation of a less circle, whose radius is i, whose centre is at the point $\lambda = \frac{\pi}{2}$ and $\kappa = 0$.

The general equation becomes still more convenient than in (5.), if we take the pole midway between the foci, and the prime meridian at right

Fig. 19.



angles to the circle joining the foci. For, then, we have $a_{,} = a_{,,}$ ($= a$ suppose), and $\beta_{,} = -\frac{\pi}{2}$, $\beta_{,,} = +\frac{\pi}{2}$. Then,

$$\cos \overline{\theta - \beta_{,}} = \cos \overline{\theta + \frac{\pi}{2}} = -\sin \theta, \text{ and } \cos \overline{\theta - \beta_{,,}} = \sin \theta.$$

Hence (8.) becomes

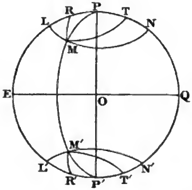
$$\sin^2 i \cos^2 i = \cos^2 \alpha \sin^2 i \cos^2 \phi + \sin^2 \alpha \cos^2 i \sin^2 \theta \sin^2 \phi, \ldots\ldots\ldots (8.)$$

From which

$$\sin \phi = \pm \frac{\sin i \sqrt{\cos^2 i - \cos^2 \alpha}}{\sqrt{\sin^2 \alpha \cos^2 i \sin^2 \theta - \cos^2 \alpha \sin^2 i}}, \ldots\ldots\ldots\ldots\ldots\ldots (8.)$$

The + and — as before referring to the two branches of the curve about the two poles.

FIG. 20.



Recurring to the formulæ for *orthographic projection* (XV. 5.) we have

$$\sin^2 \phi = \frac{v^2}{a^2}; \;\; \cos^2 \phi = \frac{a^2 - v^2}{a^2}; \text{ and } \sin^2 \theta = \frac{y^2}{v^2}.$$

These inserted in (7.) give

$$y^2 (\sin^2 i \cos^2 \alpha - \cos^2 i \sin^2 \alpha) + x^2 \cos^2 \alpha \sin^2 i = a^2 \sin^2 i \, (\cos^2 \alpha - \cos^2 i) \ldots\ldots (9.)$$

Now, since i is always greater than α, the *factors* of the equation (9.) are essentially +, for they may be written,

$$y^2 \sin (i - \alpha) \sin (i + \alpha) + n^2 \cos^2 \alpha \sin^2 i = a^2 \sin^2 i \, (\sin i - \alpha) \sin (i + \alpha) \ldots\ldots (10.)$$

This projection, therefore, is an ellipse, whose major and minor semi-axes are respectively

$$\pm a \sin i, \quad \text{and} \quad \pm a\sqrt{1 - \cos^2 i \sec^2 \alpha}.$$

XXV.

SPHERICAL PARABOLA.

There is yet one particular case of the general equation (3.) which claims attention, on account of its being related to the spherical ellipse and hyperbola, as the parabola is to the ellipse and hyperbola *in plano*. I mean when $\alpha_, \beta_,$ is one of the poles, and $i = \frac{\pi}{2}$. In this case, the distance of any point in the circumference, from the focus $\alpha_, \beta_,$, is equal to its distance from the equator *.

In this case $\beta_,$ will be indeterminate, and $\alpha_, = n\pi$, where n is a whole number. The equation (3.) becomes, after slight and obvious deductions,

$$\tan\phi = \pm \frac{\cos\alpha_{,,}}{1 \mp \sin\alpha_{,,} \cos\overline{\theta - \beta_{,,}}} \quad \ldots\ldots \quad (1.)$$

The gnomonic projection of which is an ellipse, having the pole of the hemisphere for one of its foci. See Repository already referred to. But, in addition to that, we remark, that, in uniformity with the general determination in the last article, there is also a plane on which it may give an ellipse by orthographic projection, though a different plane from the one to which in the Repository the projections were referred.

The relations we have assumed, viz. $\alpha_, = n\pi$, show that this property holds, *whichever of the poles* be taken as one of the foci. In case of $\alpha_{,,} \beta_{,,}$ being on the hemisphere which contains $\alpha_, \beta_,$, then it is the *sum* of the arcs that are equal to $\frac{\pi}{2}$; and when $\alpha_, \beta_,$ and $\alpha_{,,} \beta_{,,}$ are on different hemispheres, it is the *difference* of the arcs that make that sum.

* In the Mathematical Repository, vol. v. p. 240, pt. 1, are solutions of this case, which had been proposed some years before by Professor WALLACE. How far that gentleman had carried his inquiries into this subject, or whether he had systematically entered upon it at all, does not appear from the Repository; nor have I other means of ascertaining.

We remark, in closing, also, another simplification of the general formula, viz. when $\cos 2i = \pm 1$. Here we have (3.) converted into

$$\cot\phi = -\frac{1}{\cos\alpha_{,} \pm \cos\alpha_{,,}} \cdot \{\sin\alpha_{,} \cos\overline{\theta - \beta_{,}} \pm \sin\alpha_{,,} \cos\overline{\theta - \beta_{,,}}\} \quad \dots \; (2.)$$

which represent two great circles perpendicular to one another,—a result which possesses claims to attention, on account of some consequences derivable from it, as well as from its own geometrical beauty.

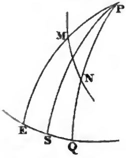
The chief modifications which (3.) can undergo are easily traced, and will all be found to depend upon the constants employed becoming, separately, or in connexion, of the form $\frac{n\pi}{2}$, where n is a whole number. But as they are easily followed out, it is unnecessary here to discuss the question at greater length.

XXVI.

Given the perimeter of a spherical triangle, together with the magnitude and position of its vertical angle, to find the curve to which its base is always a tangent.

Let EPQ be the given vertical angle, and PS, the great circle bisecting it, be taken for the origin of θ, whilst P is the origin of ϕ. Put

Fig. 21.



$EPQ = 2\kappa$, and perimeter $= 2\sigma$. Let the sides of any one of the triangles so constituted be denoted by $\alpha_{,}$, $\alpha_{,,}$ respectively. Then, by a well known theorem,

$$\sin^2\kappa = \frac{\sin\overline{\sigma - a_{\prime}}\,\sin\overline{\sigma - a_{\prime\prime}}}{\sin a_{\prime}\,\sin a_{\prime\prime}}$$

which expanded, becomes

$$(\cot a_{\prime\prime} - \cot\sigma)(\cot a_{\prime} - \cot\sigma) = \operatorname{cosec}^2\sigma\,\sin^2\kappa \;\;\ldots\ldots\ldots\; (1.)$$

Also, if for $\beta_{\prime}$ and $\beta_{\prime\prime}$ in (9. IV.) we put $-\kappa$ and $+\kappa$, we shall readily obtain the equation of the great circle MN; viz.

$$\cot\phi\,\sin 2\kappa = \sin\overline{\kappa+\theta}\,\cot a_{\prime\prime} + \sin\overline{\kappa-\theta}\,\cot a_{\prime} \;\;\ldots\ldots\ldots\; (2.)$$

Eliminating one of these cotangents, as $\cot a_{\prime\prime}$ from (1.) (2.) we find

$$\cot^2 a_{\prime} - 2\cos\kappa\,\frac{\cot\phi\,\sin\kappa - \cot\sigma\,\sin\theta}{\sin\overline{\kappa-\theta}}\cot a_{\prime} + \frac{(1-\operatorname{cosec}^2\sigma\cos^2\kappa)\sin\overline{\kappa+\theta} - \cot\phi\,\cot\sigma\,\sin 2\kappa}{\sin\overline{\kappa-\theta}} = 0 \;\ldots\; (3.)$$

The differential of (3.), taken with respect to the arbitrary quantity $\cot a_{\prime}$, gives the equation of the base consecutive to MN. It is

$$\cot a_{\prime} = \frac{\cos\kappa\,(\cot\phi\,\sin\kappa - \cot\sigma\,\sin\theta)}{\sin\overline{\kappa-\theta}} \;\;\ldots\;\; (4.)$$

Eliminating $\cot a_{\prime}$ between (3.) and (4.) we obtain

$$\cos^2\kappa\,(\cot\phi\,\sin\kappa - \cot\sigma\,\sin\theta)^2 - \cot\sigma\,\cot\phi\,\sin 2\kappa\,\sin\overline{\kappa-\theta} = (1-\operatorname{cosec}^2\sigma\cos^2\kappa)(\cos^2\theta - \cos^2\kappa) \;\ldots\; (5.)$$

Expand the vinculated quantities, and add $(\sin^2\kappa\cos^2\theta - \sin^2\theta\cos^2\kappa)\cot^2\sigma$ to both sides, and we shall find

$$(\cot\phi\,\cos\kappa - \cot\sigma\,\cos\theta)^2 = (\cos^2\theta - \cos^2\kappa)\operatorname{cosec}^2\sigma \;\;\ldots\ldots\; (6.)$$

Or extracting and transposing,

$$\cot\phi = \frac{\cos\sigma\,\cos\theta \pm \sqrt{\cos^2\theta - \cos^2\kappa}}{\cos\kappa\,\sin\sigma} \;\;\ldots\ldots\ldots\; (7.)$$

This is the equation of a circle, resolved according to $\cot\phi$ *. *Hence the locus sought is a circle*, as is well known from geometrical considerations.

It may, however, be readily brought under the more familiar form (I. 1.) as follows. Resume (6.), expand and multiply all the terms by $\sin^2\sigma\,\sin^2\phi$.

* See Note (A) at the end.

Hence,

$$\cos^2\phi\cos^2\kappa\sin^2\sigma - 2\cos\phi\sin\phi\cos\sigma\sin\sigma\cos\kappa\cos\theta + \cos^2\sigma\sin^2\phi\cos^2\theta = (\cos^2\theta - \cos^2\kappa)\sin^2\phi \quad \dots \quad (8.)$$

Or, subtracting $\cos^2\theta\sin^2\phi + \cos^2\kappa\cos^2\phi$ from both sides, and changing all the signs of the result, we obtain, by extraction,

$$\cos\phi\cos\kappa\cos\sigma + \sin\phi\sin\sigma\cos\theta = \pm\cos\kappa \quad \dots \quad (9.)$$

This is the equation of a circle, whose centre $\kappa_,\ \lambda_,$ *and radius* $\varrho_,$ *are determined by the following:*

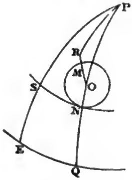
$$\left.\begin{aligned} \kappa_, &= 0 \\ \tan\lambda_, &= \tan\sigma\sec\kappa \\ \cos\varrho_, &= \frac{\pm\cos\kappa}{\sqrt{1-\sin^2\kappa\cos^2\sigma}} \end{aligned}\right\} \quad \dots \quad (10.)$$

XXVII.

SPHERICAL EPICYCLOID.

Let SN be the directrix, NM the generatrix, and R the tracing point. Join RO cutting the circle NM in M, and let S be that point of the directrix which was in contact with M. Put $PN = R$, $NO = r$, and $OR = \varrho$; put also $SPR = \theta$, $SPN = \chi$, and $PR = \phi$.

FIG. 22.



Then, since $\widehat{SN} = \widehat{NM}$, and the radii of the circles of which these are segments are as $\sin R$ to $\sin r$, we have $\widehat{SN} = \widehat{MN} = \chi\sin R$; and when

the lengths of arcs in two circles are equal, the angles subtended by them are inversely as their radii. Hence $\angle \mathrm{MON} = \chi \dfrac{\sin \mathrm{R}}{\sin r}$.

$$\text{Now, } \cos \phi = \cos \overline{\mathrm{R} \pm r} \cos \rho - \sin \overline{\mathrm{R} \pm r} \sin \rho \cos \left\{ \chi \frac{\sin \mathrm{R}}{\sin r} \right\}, \text{ or}$$

$$\chi = \frac{\sin r}{\sin \mathrm{R}} \cos^{-1} \left(\frac{- \cos \phi + \cos \overline{\mathrm{R} \pm r} \cos \rho}{\sin \overline{\mathrm{R} \pm r} \sin \rho} \right) \ldots\ldots\ldots (1.)$$

$$\text{Again, } \cos \rho = \cos \phi \cos \overline{\mathrm{R} \pm r} + \sin \phi \sin \overline{\mathrm{R} \pm r} \cos (\chi - \theta), \text{ or}$$

$$\chi = \theta + \cos^{-1} \frac{\cos \rho - \cos \phi \cos \overline{\mathrm{R} \pm r}}{\sin \phi \sin \overline{\mathrm{R} \pm r}} \quad \ldots\ldots\ldots \quad (2.)$$

Equating the values of χ given in equations (1.) and (2.), we obtain the general equation of the epicycloid; viz.

$$\theta = \frac{\sin r}{\sin \mathrm{R}} \cos^{-1} \frac{- \cos \phi + \cos \overline{\mathrm{R} \pm r} \cos \rho}{\sin \overline{\mathrm{R} \pm r} \sin \rho} - \cos^{-1} \frac{\cos \rho - \cos \phi \cos \overline{\mathrm{R} \pm r}}{\sin \phi \sin \overline{\mathrm{R} \pm r}} \ldots\ldots \quad (3.)$$

This equation is the only one in which the variables are separated that I have been able to obtain; but as the properties of the curve are neither numerous nor important, I have not been very solicitous to protract my inquiries on the subject. Perhaps, indeed, it is well for science that this curve offered so little to tempt inquiry in the early period of mathematico-philosophical history, as it is probable that the celestial machinery of Ptolemy and Tycho would not then have engaged the attention of astronomers till a much later period than they did. The complex character of the inquiries to which that system led in its geometrical details, induced astronomers to wish —to hope—to search for—some simple method of explaining the motions of the heavenly bodies. The "harmony" of the elliptic, contrasted with the "discord" of the epicycloid motions, speedily obtained for the former the preference of reflecting minds, even before any satisfactory reason had been given *why* it should be the true system. Indeed, the epicycloid system offered a closer and simpler interpretation of the phenomena, so long as the fundamental dogma of the Ptolemaic astronomy was admitted; and certainly upon the face of the inquiry this was the most obvious and natural hypothesis. Had, therefore, the epicycloid been a curve capable of gratifying Kepler's thirst for analogies and harmonies with respect to properties

both numerical and geometrical, we might not have been at this day in possession of his three celebrated laws—nor, therefore, of the theory of gravitation—of the Principia—or of the Mécanique Céléste. Our theory of the Planets, and especially of the Moon, and consequently our whole *art* of navigation, and apparatus for conducting it—and hence, our commerce, and our mechanical arts—would have been in a much less advanced state than they now are. Is it not easy to see, that upon the high degree of perfection of the mechanical arts, which are produced by extended commercial relations, the perfection of our philosophical instruments depends—that our data for testing hypotheses already framed, and to serve as conditions to be fulfilled by future theories, depends entirely upon that excellence of workmanship which a high state of commerce alone can call into being? Where, indeed, would have been our *precision* in all the sciences—and what probability is there that we should have entertained correct and legitimate views upon any one of them? It would be a curious and interesting amusement to speculate upon the probable state, at the present time, of physical and practical astronomy—of the doctrines of sound and light—of meteorology—of chemistry—of electro-magnetism—and even of mathematical science itself—had the spherical epicycloid possessed a numerous and interesting assemblage of geometrical properties.

There is, however, one point of view in which it possesses considerable interest—*that individuals of this curve traced on the surface of the sphere are capable of rectification.* The Florentine paradox proposed to find *quadrable portions of the sphere;* and it could scarcely escape the geometers who considered VIVIANI's problem, to inquire into the possibility of finding curves whose contours should be equal to given straight lines. The question does not appear to have been publicly hazarded till more than twenty years afterward, when it was proposed by M. OFFENBERG, in the Leipsic Acts for 1718. I do not find that the proposer ever offered any solution, and it is probable, either that he was not in possession of one, or that he afterwards discovered the fallacy of that which he supposed himself to have found before he proposed the question. HERMANN, however, was led, by the property of the plane epicycloid being in certain cases rectifiable, to suppose the same thing held in spherical ones, under similar circumstances;

and he published, in the first volume of the Petersburg Commentaries, an attempted demonstration of it *. John Bernoulli, in the Memoirs of the Royal Academy, 1732 †, exposed the fallacy of this supposition, and pointed out the paralogism committed in the reasoning. He at the same time gave a general expression for the length, and deduced from it a particular case, which actually did solve the problem of Offenberg ‡. By a singular coincidence—*if, indeed, it were really accidental*—the attention of Maupertius, Nicole, and Clairault, were directed to the same question, and they obtained the same kind of results. Their investigations are printed in the same volume of the *Mémoires* as that of Bernoulli §.

We perceive, by inspecting our equation (3), that the curve is algebraical or transcendental, according to the commensurability of $\sin R$ to $\sin r$, just as the case with the directing and generating radius of the epicycloid *in plano*. To ascertain the conditions of capability of rectification, we may form the differential equation of the curve; which is done by differentiating (3). Thus we have

$$\left.\begin{aligned} d\theta = &-\frac{\sin r}{\sin R}\cdot\frac{\sin\phi\, d\phi}{\sqrt{(\sin^2\overline{R\pm r}\sin^2\rho - \cos^2\overline{R\pm r}\cos^2\rho) + 2\cos\overline{R\pm r}\cos\rho\cos\phi - \cos^2\phi}} \\ &+\frac{\operatorname{cosec}\phi\,(\cos\overline{R\pm r} - \cos\rho\cos\phi)\, d\phi}{\sqrt{(\sin^2\overline{R\pm r} - \cos^2\rho) + 2\cos\overline{R\pm r}\cos\rho\cos\phi - \cos^2\phi}} \end{aligned}\right\} (4.)$$

One essential condition of $\int\sqrt{\sin^2\phi\, d\theta^2 + d\phi^2}$ becoming rectifiable, is, that the quantity under the radical shall become a perfect square; and, as

* There is a copy of Hermann's dissertation inserted in John Bernoulli's works, vol. iii. p. 211.

† Page 240; also his works, vol. iii. p. 220.

‡ Mém. de l'Acad. 1732, p. 243. Opera Omnia, tom. iii. p. 223.

§ Maupertius, p. 255: Nicole, p. 271: Clairault, p. 289. It is rather singular that Dr Young, in his Catalogue, refers to the paper of Maupertius, but does not mention those of Nicole and Clairault, which follow them in the same volume. Dr Young mentions a paper on Spherical Epicycloids, by Lexell, in the third volume of the Petersburg Acts; but I have no means of consulting that series of Mémoires, there not being a copy in this city.

applied to the question before us, it requires, amongst other conditions, that the denominators in the two terms which compose the value of $d\theta$ in equation (4) shall become identical*. This is quite independent of the curve being algebraic or transcendental (that is, as before stated, independent of the ratio of sin R to sin r), by making $\rho = \frac{\pi}{2}$, which reduces the expression to

$$d\theta = -\frac{\sin r}{\sin \mathrm{R}} \cdot \frac{\sin\phi\, d\phi}{\sqrt{\sin^2 \overline{\mathrm{R} \pm r} - \cos^2\phi}} + \frac{\cos \overline{\mathrm{R} \pm r} \operatorname{cosec}\phi\, d\phi}{\sqrt{\sin^2 \overline{\mathrm{R} \pm r} - \cos^2\phi}}$$

$$= \frac{\dfrac{\sin \mathrm{R} \cos \overline{\mathrm{R} \pm r} - \sin r \sin^2\phi}{\sin \mathrm{R} \sin\phi}}{\sqrt{\sin^2 \overline{\mathrm{R} \pm r} - \sin^2\phi}} \text{..........................} (5.)$$

Inserting this in $\pm\sqrt{\sin^2\phi\, d\theta^2 + d\phi^2}$, we obtain the element of epicycloidal arc

$$d\mathrm{L} = \pm\frac{\sin\phi\, d\phi}{\sin \mathrm{R}} \sqrt{\frac{\sin^2 r \sin^2\phi + \sin^2 \mathrm{R} - 2\sin \mathrm{R} \sin r \cos \overline{\mathrm{R} \pm r}}{\sin^2\phi - \cos^2 \overline{\mathrm{R} \pm r}}} \text{......} (6.)$$

The radical of (6) can only become rational when

$$2\sin \mathrm{R} \sin r \cos \overline{\mathrm{R} + r} - \sin^2 \mathrm{R} = \sin^2 r \cos^2 \overline{\mathrm{R} \pm r} \text{..........} (7.)$$

This divides itself into two cases, which must be considered separately, namely, according as $\overline{\mathrm{R} \pm r}$ or $\overline{\mathrm{R} - r}$ is taken.

1. Take $\overline{\mathrm{R} + r}$: then expanding and reducing with respect to sin R, we have

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* Perhaps it would be more accurate to say the one should be *a multiple* of the other by a *constant factor*: but it will be found that this introduces a complexity into the expression, which is fatal to the *subsequent rationalization* of the general formula for the arc. The result of such a supposition is an *imaginary* quadratic. However, as upon this hinges the only doubt respecting the general conclusion in the subsequent investigation, I shall enter upon it with all requisite detail on some future occasion.

$$\sin^2 R = \frac{\sin^2 r \cos^2 R}{(1+\sin^2 r)^2 \mp 2(1+\sin^2 r)\sin r \cos r\sqrt{-1}} \dots\dots\dots\dots\dots\dots (8.)$$

which, being *imaginary*, shows that no such relation can exist.

2. Take $\overline{R-r}$: then, after reducing in the usual manner, we find the relation

$$\sin^2 R - \sin^2 r = 0 \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots (9.)$$

which shews that the said condition is fulfilled by arcs which are either equal or supplementary. But if we put this in (5), we shall have $d\theta = k\sqrt{-1}$, which shows that $d\theta$ does not *now* depend upon ϕ or its differentials, and hence, that the length of the epicycloid is not, *in this case*, a function of ϕ; as indeed we know it should not, ϕ being constant, while θ is variable. Indeed, if we put this value in (6), we find, that whilst it fulfils the conditions imposed upon it, viz. the condition of equation (7), it also renders both the numerator and denominator of the radical in (6) *imaginary*, indicating that contradictory conditions have been introduced amongst the data: and we readily discover that the contradiction is, the supposed dependence of θ upon ϕ, whilst $\sin^2 R - \sin^2 r = 0$. This result was necessary, in order to fully indicate all the circumstances of the inquiry, as, had the second side of (5) merely vanished, one should have inferred (legitimately too) that θ was constant; that is, it would have indicated an error.

All attempts to accomplish this object by means of a relation between R and r, therefore, failing, we are led to consider the only possible way in which it might be effected,—that is, by considering R and r as *independent quantities*, or, in other words, taking some fixed value of one of them, whilst the other is perfectly arbitrary, which would fulfil the conditions we have been seeking for. To perform this, we have only to expand (7) in terms of some one function of R, and again in terms of some one function of r. We shall thus get, considering R the arbitrary,

$$\text{for } \overline{R+r}, \left\{ \begin{array}{l} \sin^4 r \cos^4 r - 2 \sin r \cos r\,(1+\sin^2 r)^2 \sin^2 R + \\ +(1+\sin^2 r)^2 \{(1+\sin^2 r)^2 + 4\sin^2 r \cos^2 r\} \sin^4 R = 0. \end{array} \right\} \dots\dots\dots (10.)$$

$$\text{for } \overline{R-r}, \quad \cos^4 r \sin^2 R - \cos^4 r \sin^2 r = 0 \dots\dots\dots\dots\dots\dots\dots\dots\dots\dots (11.)$$

If we consider r the arbitrary, we shall have the equations

$$\text{for } \overline{R+r}, \quad \begin{cases} \sin^8 r\,(5\sin^4 R + 2\sin^2 R - 1) + \sin r\,(\sin^2 K + 4\sin^4 K) \\ + \sin^4 r\,(7\sin^4 R - 2\sin^2 R + 1) + \sin^2 r\,(8\sin^4 R - 2\sin^2 R) \\ + \sin^4 R = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (12.) \end{cases}$$

$$\text{for } \overline{R-r}, \quad \sin^4 r - 2\sin^2 R \sin^2 r + \sin^4 R = 0 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (13.)$$

Now, that the proposed object may be attainable at all, we must have one of the following sets of conditions fulfilled.

From equation (10.)

$$\left.\begin{array}{ll} 0 = \sin^4 r \cos^4 r \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & a, \\ 0 = \sin r \cos r\,(1 + \sin^2 r) \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & b, \\ 0 = (1 + \sin^2 r)^2 \{(1 + \sin^2 r)^2 + 4\sin^2 r \cos^2 r\} \ldots\ldots & c, \end{array}\right\} \ldots\ldots\ldots\ldots (14.)$$

From equation (11.)

$$\left.\begin{array}{ll} 0 = \cos^4 r \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & a_{\prime\prime} \\ 0 = \cos^4 r \sin^2 r \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & b_{\prime\prime} \end{array}\right\} \ldots\ldots\ldots\ldots (15.)$$

From equation (13.)

$$\left.\begin{array}{ll} 0 = 5\sin^4 R + 2\sin^2 R - 1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & a_{\prime\prime\prime} \\ 0 = 4\sin^4 R + \sin^2 R \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & b_{\prime\prime\prime} \\ 0 = 7\sin^4 R - 2\sin^2 R + 1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & c_{\prime\prime\prime} \\ 0 = 8\sin^4 R - 2\sin^2 R \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & d_{\prime\prime\prime} \\ 0 = \sin^4 R \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & e_{\prime\prime\prime} \end{array}\right\} \ldots\ldots\ldots\ldots (16.)$$

From equation (14.)

$$\left.\begin{array}{ll} 0 = 1 \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & a_{\text{IV}} \\ 0 = 2\sin^2 R \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & b_{\text{IV}} \\ 0 = \sin^4 R \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots & c_{\text{IV}} \end{array}\right\} \ldots\ldots\ldots\ldots (17.)$$

In the set (14), equation $a,$ is separately fulfilled (and fulfilled *alone*) by

$$\sin r = 0, \text{ and } \cos r = 0.$$

Equation $b,$ is fulfilled also by each of these: but equation $c,$ is not fulfilled by either of them. Hence, so long as in the spherical epicycloid we employ $\overline{R+r}$, there is no value of r that will leave R arbitrary, and yet fulfil the conditions required to render (6) rational.

In the set (15), both equations $a_{\prime\prime}$ and $b_{\prime\prime}$ are fulfilled by the same value

$$\cos r = 0, \quad \text{or } r = \frac{\pi}{2}.$$

There is hence, in this case, a possibility of rationalizing equation (6), *whilst* R *remains perfectly arbitrary.* Here, the element of the length of the epicycloid is

$$= \pm \frac{\sin \phi \, d\phi}{\sin R} \quad \text{.................................(18.)}$$

This is, in fact, the solution of John Bernoulli, and agrees with his result. We shall return to this expression presently.

In the set (16), we have equation $e_{\prime\prime\prime}$ fulfilled only by $\sin R = 0$, or $R = n\pi$; and $b_{\prime\prime\prime}$, $d_{\prime\prime\prime}$ are also fulfilled by the same. But $a_{\prime\prime\prime}$ and $c_{\prime\prime\prime}$ are not fulfilled by that value of R: and hence, on the hypothesis of $\overline{R + r}$ being taken, r cannot be rendered indeterminate by any value whatever of R.

In the set (17), too, we have the absurdity $1 = 0$, given by equation $a_{\prime\prime}$. This, then, is also impossible.

Hence we have proved that John Bernoulli's *is the* only *solution possible, of finding a spherical epicycloid accurately rectifiable:* so long, at all events, as the tracing point is in the periphery of the rolling circle.

In the numerical evaluation of the integral of equation (18), we have to recur to some considerations respecting the conditions of the formation of (11, 12, 13, 14). It will be recollected that these are the expansions of $\cos \overline{R \pm r}$, in which we have worked as if R were the greater radius-vectors, or, in other words, as if the direction were the tropic most remote from the pole of reference P. But as the results of all the operations we have hitherto performed are the same as if we had employed $\cos \overline{R + r}$ instead of $\cos R \pm r$, our conclusions apply equally to one case as to the other. We may, however, consider that the above expression (18) is adapted alike to both cases. The double sign attached to (18) indicates that the length may be estimated either way from the origin of the arc, that is, the same way as we have estimated θ, or the reverse. Put the length $= L$: then

$$\frac{\pm \cos \phi + c}{\sin R} = L \quad \text{.................................(19.)}$$

Now, whether R be greater or less than $\frac{\pi}{2}$, we have, when $\phi = \mathrm{R}$, the length $= 0$; for that is the point which we have taken as origin. Hence

$$c = \pm \cos \mathrm{R};$$

and we have, finally,

$$\pm \left(\frac{\cos \phi - \cos \mathrm{R}}{\sin \mathrm{R}}\right) = \mathrm{L} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (20.)$$

If, now, instead of considering the curve as referred to polar co-ordinates, we transform it into a rectangular equation referred to the equator: then we shall have (20) converted into

$$\mp \left(\frac{\sin \phi - \sin \mathrm{R}}{\cos \mathrm{R}}\right) = \mathrm{L} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (21.)$$

If, also, instead of considering the origin to be at the point of contact of the epicycloid with the tropic, we had taken it as the intersection with the meridian: then when $\phi = 0$, we should have $\mathrm{L} = 0$, and hence

$$c = 0,$$

and the length of the curve would simply be

$$\mp \frac{\sin \phi}{\cos \mathrm{R}} = \mathrm{L} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (22.)$$

The length of the curve estimated from the equator, then, varies with the sine of declination of the corresponding point in it; a property so singular in the estimation of BERNOULLI, that he did not think another curve could exist having that property*.

The equation (3), on the hypothesis of $r = \rho = \frac{\pi}{2}$, is transformed into

$$\theta \sin \mathrm{R} = \cos^{-1} \frac{-\cos \phi}{\mp \cos \mathrm{R}} - \sin \mathrm{R} \cos^{-1} \cot \phi \tan \mathrm{R} \ldots\ldots\ldots\ldots (23.)$$

This curve, then, is generally a transcendent upon a spherical surface,

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* *Vide* BERN. Op. om. iii. p. 233.; or Mém. de l'Acad. 1732, p. 234. The statement above of the property is not exactly in form the same as that given by BERNOULLI. He calls the "abscissa" the versed sine of the arc of the rolling circle which has already been in contact. This versed sine is in the *plane* of the circle itself, and therefore is to the

(that is, it is a transcendent in the same cases as the plane curve), and yet has the unexpected property of being rectifiable: but it is by no means a singular case, for innumerable curves may be traced on a sphere combining the two properties. The general condition of rectification, indeed, involves an arbitrary function, whichever of the variables be eliminated; and hence it is easy to see that the property must be a very extensive one. I cannot, however, enter into details here, but shall reserve what I have to say on this topic till a future occasion.

Note.—The most scientific *geometrical* view of the spherical epicycloid has been given by Hachette, in the Correspondence of the Polytechnic School, vol. ii. p. 24.; in his supplement to the Descriptive Geometry of Monge, p. 88.; and in his own treatise on the same subject, p. 159. It might also be mentioned, that Clairault* has exhibited quadrable portions of the spherical epicycloid; and that, did our limits permit, we might effect the same very elegantly by our own methods. It is not necessary here, however, to discuss that question.

XXVIII.

THE SUN'S VERTICAL PATH OVER THE EARTH.

An elegant problem was proposed by John Bernoulli in 1732, which has a close analogy to this inquiry, a solution of which we shall set down.

What is the path of the vertical projection of the sun upon the earth's surface, supposing the sun to move round the earth in a circle with an equable motion, and the earth itself to be a sphere?

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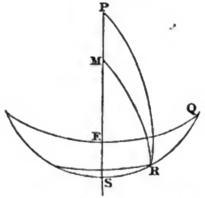
sine of the declination as sine declination is to radius. Making these transformations, the result I have given above will be found identical with Bernoulli's, though obtained by so completely different a process.

* Mém. de l'Acad. des Scien. 1732, pp. 293-4.

Let EQ be the equator, P its pole; SR the ecliptic, and M its pole. Refer the whole system to the equator and the meridian through the tropic of Capricorn. Then S in PM is that tropic, and PM is the measure of the inclination λ of the ecliptic to the equator.

FIG. 23.



The equation of the ecliptic is, then, if $\text{SPR} = \theta_{,}$ and $\text{PR} = \phi_{,}$

$$\cot \phi_{,} = -\tan \lambda \cos \theta_{,} \ldots\ldots (1.)$$

But if $\text{SR} = \chi = \text{SMR}$, we have

$$\cos \text{SR} = \cos \text{SP} \cos \text{PR} + \sin \text{SP} \sin \text{PR} \cos \text{SPR}, \text{ or}$$

$$\cos \chi = \cos \left(\frac{\pi}{2} + \lambda\right) \cos \phi_{,} + \sin \left(\frac{\pi}{2} + \lambda\right) \sin \phi_{,} \cos \theta_{,}, \text{ or}$$

$$\cos \chi = -\cos \phi_{,} \sin \lambda + \cos \lambda \sin \phi_{,} \cos \theta_{,} \ldots\ldots (2.)$$

$$\text{or } \chi = \cos^{-1} \{-\cos \phi_{,} \sin \lambda + \cos \lambda \sin \phi_{,} \cos \theta_{,}\} \ldots\ldots (3.)$$

But in the present case, if ϕ, θ be the co-ordinates of the curve sought, we have $\phi = \phi_{,}$, and hence from (1) we have $\cos \theta_{,} = -\cot \phi \cot \lambda$, and this put in (3), gives

$$\chi = \cos^{-1} \{-\cos \phi \sin \lambda - \cos \lambda \sin \phi \cot \phi \cot \lambda\}$$

$$= \cos^{-1} \left\{-\frac{\cos \phi}{\sin \lambda}\right\} \ldots\ldots (4.)$$

But if n express the ratio of the earth's angular velocity upon its axis to its angular velocity about the sun, we shall have $\theta = n\chi$, which, inserted in (4), gives us

$$\theta = n\chi = n\cos^{-1}\left(\frac{-\cos\phi}{\sin\lambda}\right), \text{ or}$$

$$\cos\phi = -\sin\lambda\cos\frac{1}{n}\theta \dots\dots\dots\dots (5.)$$

The equation of the curve is thus found to be very simple: and it may be further remarked, that if $\lambda = \frac{\pi}{2}$, it becomes the equable spherical spiral already considered. For then

$$\sin\lambda = 1, \text{ and } \frac{1}{n}\theta = \cos^{-1}(-\cos\phi) = \pi - \phi,$$

which differs from the equations already found only in being referred to a different origin either of ϕ or θ. We may consider it to refer to ϕ measured from the opposite pole, or to θ measured from the opposite meridian. In our equation of the circle, too, the same result is indicated, and we might therefore look for it here.

The equation (5) given above may, indeed, be considered as the most general form of the *equable spherical spiral*, and from which all the particular cases (as the spiral of Pappus, the oval window of Viviani, &c.) may be at once derived. The discussion of the question under this aspect might be interesting, but, after the detail into which we have already entered, it is not necessary to do it here.

XXIX.

The proposal of this curve, however, originated in a mistaken analogy between this combination of motion and that which produces the spherical epicycloid*. About the same time, Maupertius† proposed and offered a solution to a problem having some similarity to this, viz. to find that meridian on the celestial sphere, where the motion in right ascension and in longitude have a given ratio to one another; and it had been discussed al-

* Mém. de l'Acad. 1732, p. 245; or Opera Omnia, tom. iii. p. 226.

† Mém. de l'Acad. 1732, pp. 257-8.

ready, in the case of a ratio of equality by MM. Parent * and Godin †, as it has also been by most authors who have written on such subjects since. The following solution is different, I think, from any that have preceded it; and though it might, in some points of view, be deemed very elementary, yet as it is short, it might not be improperly introduced here, as another specimen of the method. It may be remarked that Godin professes to dispense with the differential calculus; but he does so *only in form*, as the elementary spherical triangle is in reality the one that is employed in his method "*par les Infiniments Petits.*"

The circles being represented as above, we have

$$\cos\chi = -\cos\phi\sin\lambda + \cos\lambda\sin\phi\cos\theta \quad\ldots\ldots\ldots\ldots(1.)$$

$$\cot\phi = -\tan\lambda\cos\theta \quad\ldots\ldots\ldots\ldots(2.)$$

$$n\,d\theta = d\chi \quad\ldots\ldots\ldots\ldots(3.)$$

The first of these is the expression for the arc of the ecliptic intercepted between the first point of Capricorn and a point in the ecliptic, whose co-ordinates are ϕ, θ. The second is the equation of the ecliptic, referred to the equator and winter colure; and the third is the given relation between the velocities of the sun in right ascension and longitude.

From (1, 2) we get

$$\cos\chi = -\frac{\cos\phi}{\sin\lambda} \quad\ldots\ldots\ldots\ldots\ (4.)$$

$$\text{and hence } d\chi = -\frac{\sin\phi\,d\phi}{\sqrt{\sin^2\lambda - \cos^2\phi}} \quad\ldots\ldots\ (5.)$$

Differentiating (2), we find

$$d\theta = \frac{\cos\lambda\,d\phi}{\sin\phi\sqrt{\sin^2\lambda - \cos^2\phi}} \quad\ldots\ldots\ldots\ (6.)$$

Inserting (5, 6) in (3), we get

$$\sin^2\phi = n\cos\lambda \quad\ldots\ldots\ldots\ldots\ (7.)$$

which gives the north polar distance of the sun at the time in question, and agrees with the results obtained by other methods.

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* Mem. 1704, p. 315. † 1730, p. 27.

Combining (7) with (4) and (2), we get the sun's distance from the tropic of Capricorn, and hence from the beginning of Aries; and likewise his right ascension at the time.

XXX.

THE LOXODROME, OR RHUMB-LINE.

There is no one curve, perhaps, in the whole compass of that infinite variety which presents itself to us, which possesses so many interesting claims upon the attention of the speculative and the practical mathematician. The first in all probability that was made the object of consideration and inquiry upon the surface of the sphere, and, except the conic sections, upon any curve surface whatever—capable of very simple and direct investigation, and well calculated to suggest to the mind the nature of analytical geometry on the sphere, such as we have here considered—a curve of the very greatest importance in the practice of navigation, and, therefore, upon which the commerce of the world, and the comforts and luxuries of civilised life, in a great degree depended—a curve possessed of a long train of curious geometrical properties, well worthy the attention of the accomplished mathematician, yet, from their simplicity and elegance, well adapted to discipline the mind, and cultivate the taste of students less advanced in this kind of speculation—and, finally, a curve upon the investigation of which many mistakes have been made, and, in some cases, even by mathematicians of no humble pretensions.

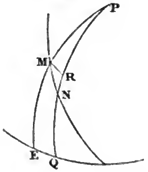
It is not my intention to enter largely into the discussion in the present paper, as it would require one or two preliminary theorems concerning spherical curvature, which will form part of a subsequent dissertation, to give the inquiry all its effect; nevertheless I shall discuss a few of the more obvious properties of this curve, though rather with a view to exhibit a few additional applications of the method of spherical co-ordinates, than to exhaust the inquiry of its interest to other mathematicians. In the course of the present session of the Royal Society's sittings, it is not improbable I may be able to furnish a continuation of the subject generally.

XXXI.

The loxodrome cuts all the meridians under the same given angle—required its spherical equation.

Let P be the pole, $\widehat{EQ}$ an element of the equator corresponding to $\widehat{EP}$, $\widehat{PQ}$, the meridians passing through the extremities M, N of an element of the curve.

Fig. 24.



Let $\widehat{MP} = \phi$, and the longitude of M from some fixed meridian $= \theta$. Then $MPN = d\theta = \widehat{EQ}$, and $\widehat{RN} = d\phi$. Hence we have

$$\widehat{MR} = EQ \cos \widehat{EM} = \sin \phi \, d\theta \quad \ldots\ldots \quad (1.)$$

But considering the ultimate elements of the arcs which form the elemental triangle MRN as straight lines, we have

$$\frac{MR}{RN} = \frac{d\theta \,.\, \sin\phi}{d\phi} = \tan \alpha \quad \ldots\ldots \quad (2.)$$

where $\alpha = MNR =$ angle of the rhumb.

From (2.) we have

$$d\theta \,.\, \cot \alpha = d\phi \,.\, \operatorname{cosec} \phi,$$

or integrating,

$$\log \tan \tfrac{1}{2}\phi = \theta \,.\, \cot \alpha + c \quad \ldots\ldots \quad (3.)$$

The value of c will depend upon the meridian which we select as the origin of θ, the most natural (and the most convenient it proves to be) is that where the loxodrome cuts the equator. This gives as simultaneous values of the variables

$$\theta = 0, \text{ and } \phi = \frac{\pi}{2},$$

Hence we have $\log \tan \frac{\pi}{4} = c$; or since $\tan \frac{\pi}{4} = 1$, $\log \tan \frac{\pi}{4} = 0$, and equation (3.) becomes simply

$$\log \tan \tfrac{1}{2}\phi = \theta \,.\, \cot a \quad \ldots\ldots \quad (4.)$$

This equation may be rendered more convenient for subsequent investigation in some other forms.

Take $\log^{-1}$ of both sides, then

$$\tan \tfrac{1}{2}\phi = \epsilon^{\cot a \,.\, \theta} = \kappa^{\theta}$$

(where ϵ is the hyp-base, and $\kappa = \epsilon^{\cot a}$).

Or squaring both sides, and expressing $\tan^2 \frac{1}{2}\phi$ in terms of $\cos\phi$, it becomes

$$\kappa^{2\theta} = \frac{1 - \cos\phi}{1 + \cos\phi}$$

Or reversing this result, we have

$$\cos\phi = \frac{1 - \kappa^{2\theta}}{1 + \kappa^{2\theta}} = \frac{\kappa^{-\theta} - \kappa^{\theta}}{\kappa^{-\theta} + \kappa^{\theta}} \quad \ldots\ldots\ldots \quad (5.)$$

$$\text{Also, } \sin\phi = \pm\frac{2\kappa^{\theta}}{1 + \kappa^{2\theta}} = \pm\frac{2}{\kappa^{-\theta} + \kappa^{\theta}} \quad \ldots \quad (6.)$$

$$\tan\phi = \pm\frac{2\kappa^{\theta}}{1 - \kappa^{2\theta}} = \pm\frac{2}{\kappa^{-\theta} - \kappa^{\theta}} \quad \ldots \quad (7.)$$

And so on with other functions of the spherical radius-vector ϕ.

XXXII.

We proceed to ascertain the course of the curve, which the equation enables us easily to perform.

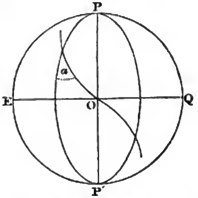
The discussion divides itself naturally into two cases, according as $\cot a$ is positive or negative, that is, as a itself is greater or less than a right angle. We shall keep the two cases on the same folio, opposed to one another, for the sake of more ready comparison. Moreover, as the variable ϕ is expressed by means of its tangent, there are, for every value of ϕ, two points in the locus, diametrically opposite to one another on the sphere, and hence we shall first trace that which is given by the least value of ϕ. We shall also take a only with first and second quadrants, as, by taking it in the third and fourth, we only get a repetition of the same angle (measured in the opposite direction, it is true, but upon the same great-circle tangent to the curve), and hence only a repetition of the same results as are obtained from the first and second. The value of $\theta = 0$, is placed in the

middle of each column, and we proceed upwards and downwards from it to the extremities of its possible values, viz. + inf. and — inf., or $+\frac{1}{0}$ and $-\frac{1}{0}$ *.

The equation of the loxodrome is $\log \tan \frac{1}{2} \phi = \theta \cot a$; and as the result is given in $\tan \phi$, the primary and secondary branches are *diametrically* opposite, or the genesis is of the *conical* kind. In what follows, let m denote any real positive number, either whole or fractional, rational or irrational; and let κ be any angle less than $\frac{\pi}{2}$. Then for the case of

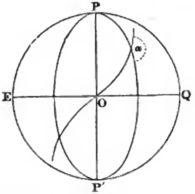
$$a = \frac{\pi}{2} - \kappa, \text{ or } a = \frac{3\pi}{2} - \kappa, \text{ we have } \cot a = +i.$$

FIG. 25. (See p. 80.)



Continuing the same hypothesis, with the exception of what relates to a, let us put $a = \frac{\pi}{2} + \kappa$, and $a = \frac{3\pi}{2} + \kappa$ whence $\cot a = -i$.

FIG. 26. (See p. 81.)



* The common form of the symbol of infinity is open to some objections. Baron FOURIER, in his Treatise on Heat, and in his posthumous work on the solution of Equations, has employed $\frac{1}{0}$, and certainly it has the advantage of expressing distinctly the arithmetical origin and signification of that quantity. It has too, some collateral advantages, which, however, it would be foreign to our subject to discuss.

$\theta =$	RESULTS.	INFERENCES. (Fig. 25.)
$-\frac{1}{0}$	$\log \tan \frac{1}{2}\phi = -\frac{i}{0}$ $\therefore \tan \frac{1}{2}\phi = 0$ $\left\{\begin{array}{l}\phi = 0 \text{ primary} \\ \phi = \pi \text{ secondary}\end{array}\right\}$	The primary branch of negative revolution passes through P, and there meets the secondary branch of positive revolution. The secondary branch of negative revolution passes through P′, and there meets the primary branch of positive revolution.
$-\frac{1}{m}$	$\log \tan \frac{1}{2}\phi = -\frac{i}{m}$ $\therefore \tan \frac{1}{2}\phi \angle 1$; $\left\{\begin{array}{l}\phi \angle \frac{\pi}{2} \text{ primary} \\ \phi \angle \frac{3\pi}{2} \text{ secondary}\end{array}\right\}$	The primary branch of negative revolution is in the first quadrant of polar distance. The secondary branch of negative revolution is in the third quadrant of polar distance.
0	$\log \tan \frac{1}{2}\phi = 0$ $\therefore \tan \frac{1}{2}\phi = 1,$ $\left\{\begin{array}{l}\phi = \frac{\pi}{2} \text{ primary} \\ \phi = \frac{3\pi}{2} \text{ secondary}\end{array}\right\}$	The primary branch of negative revolution has arrived at the origin of revolution, and the tracing point is in the equator. The secondary branch of negative revolution has arrived at the opposite meridian, and the tracing point is in the equator.
$+\frac{1}{m}$	$\log \tan \frac{1}{2}\phi = \frac{i}{m}$ $\therefore \tan \frac{1}{2}\phi > 1$; and $\left\{\begin{array}{l}\pi > \phi > \frac{\pi}{2} \text{ primary} \\ 2\pi > \phi > \frac{3\pi}{2} \text{ secondary}\end{array}\right\}$	The primary branch of positive revolution is in the second quadrant of polar distance; and The secondary branch of positive revolution is in the fourth quadrant of polar distance.
$+\frac{1}{0}$	$\log \tan \frac{1}{2}\phi = +\frac{i}{0}$ $\therefore \tan \frac{1}{2}\phi = +\frac{i}{0}$ $\left\{\begin{array}{l}\phi = \pi \text{ primary} \\ \phi = 2\pi \text{ secondary}\end{array}\right\}$	The primary branch of positive revolution has arrived at the opposite pole P′, where it unites with the secondary branch of negative revolution. The secondary branch of positive revolution has arrived at the upper pole P, where it unites with the primary branch of negative revolution.

$\theta =$	RESULTS.	INFERENCES. (Fig. 26.)
$-\frac{1}{0}$	$\log \tan \frac{1}{2}\phi = +\frac{i}{0}$ $\therefore \tan \frac{1}{2}\phi = +\frac{i}{0}$ $\left\{\begin{array}{l} \phi = \pi \text{ primary} \\ \phi = 2\pi \text{ secondary} \end{array}\right\}$	The primary branch of negative revolution has arrived at P′, where it unites with the secondary branch of positive revolution. The secondary branch of negative revolution has arrived at P, where it unites with the primary branch of positive revolution.
$-\frac{1}{m}$	$\log \tan \frac{1}{2}\phi = +\frac{i}{m}$ $\therefore \tan \frac{1}{2}\phi > 1$; and $\left\{\begin{array}{l} \pi > \phi > \frac{\pi}{2} \text{ primary} \\ 2\pi > \phi > \frac{3\pi}{2} \text{ secondary} \end{array}\right\}$	The primary branch of negative revolution is now in the second quadrant of polar distance; and The secondary branch of negative revolution is in the fourth quadrant of polar distance.
0	$\log \tan \frac{1}{2}\phi = 0$, $\therefore \tan \frac{1}{2}\phi = 1$, or $\left\{\begin{array}{l} \phi = \frac{\pi}{2} \text{ primary} \\ \phi = \frac{3\pi}{2} \text{ secondary} \end{array}\right\}$	The primary branch of negative revolution has arrived at the origin of revolution, and the tracing point is in the equator. The secondary branch of negative revolution has arrived at the opposite meridian, and the tracing point is in the equator.
$+\frac{1}{m}$	$\log \tan \frac{1}{2}\phi = -\frac{i}{m}$ $\therefore \tan \frac{1}{2}\phi < 1$; and $\left\{\begin{array}{l} \phi < \frac{\pi}{2} \text{ primary} \\ \phi < \frac{3\pi}{2} \text{ secondary} \end{array}\right\}$	The primary branch of positive revolution is in the first quadrant of polar distance; and The secondary branch of positive revolution is in the third quadrant of revolution.
$+\frac{1}{0}$	$\log \tan \frac{1}{2}\phi = -\frac{i}{0}$ $\therefore \tan \frac{1}{2}\phi = 0$ $\left\{\begin{array}{l} \phi = 0 \text{ primary} \\ \phi = \pi \text{ secondary} \end{array}\right\}$	The primary branch of positive revolution has arrived at P, and there unites with the secondary branch of negative revolution. The secondary branch of positive revolution is arrived at P′, where it unites with the primary branch of negative revolution.

It requires but a moment's reflection to shew that these two tables represent curves perfectly equal in all respects, but reversed in position. If we cause the sphere, upon which $\cot a = -i$ is traced, to turn upon PP′ half a revolution, and then to turn upon the diameter perpendicular to the plane of the paper half a revolution also, the whole system will become identical with that represented in the figure to $\cot a = +i$, provided i have the same numerical value in both cases.

To state consecutively the course of the curve, then, let us confine our attention to the former figure (25.) and its corresponding analytical table. Departing from 0 in the *negative* direction (that is, *to the left*), the curve winds round the pole, an infinite number of times; after which it passes through the pole, making with the first meridian POP′ the same angle a. It then winds round the pole P, in the positive direction (that is, to the *right*), and departing farther at each turn, cuts the equator, in a point O′, diametrically opposite to O. At this point its direction of revolution becomes negative again, and it proceeds into the other hemisphere, passes through P′ after an infinite number of circumvolutions, and returns by the positive direction round the pole through the extent of the same lower hemisphere till it arrives at the origin O, where it coalesces with the branch along which we set out. *The course of the curve is therefore completely assigned.*

We are thus enabled to answer satisfactorily a curious question that was much agitated by the earlier writers upon nautical subjects; viz. "*Whether a ship, by sailing on the same rhumb, would ever return to the place from which it set out?*" Dr WILSON, in his History of the Rise and Progress of Navigation *, tells us, that Mr JOHN BASSAT, in a posthumous work, published about 1630, established this proposition in the affirmative. Though I have long sought for that little work ("Pathway to Perfect Sailing"), I have sought in vain; and am therefore unable to give any account of the manner in which BASSAT has performed this under-

* Prefixed to Robertson's Navigation. The remark is in the foot-note at p. xv, xvi, of Wales's edition (or fourth) of that work.

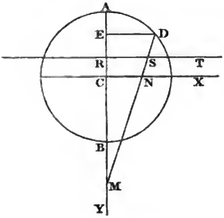
taking. I should think it a task of some difficulty, in the then state of mathematical science. However this may be, the point is fully established in the present article; and we see, that, in order to return to the same point, the ship must pass through both poles, and make two successive sets, each of an infinite number of convolutions round each of those poles, before it returns. We need not pursue the subject further here.

XXXIII.

To find the projections of the loxodrome on a plane parallel to the equator.

By (XV. 2.) we have the value of v, the radius vector, in terms of ϕ, and the constants of the projecting data; and by (XXXII.) we have the value of ϕ in terms of θ. Combining these two, we have

$$v = \frac{\pm 2a(b+c)k^{\theta}}{(a+b)-(a-b)k^{2\theta}} = \pm \frac{2a(b+c)}{(a+b)k^{-\theta}-(a-b)k^{\theta}} \quad \ldots \text{ (1.)}$$



Or,
$$\frac{1}{v} = \pm \frac{(a+b)k^{-\theta}-(a-b)k^{\theta}}{2a(b+c)} \quad \ldots\ldots\ldots \text{ (2.)}^{*}$$

* This is the general equation of Cotes's spirals: but as it does not admit of the values $k = \epsilon^{\pm \cot \alpha \sqrt{-1}}$, the sixth species cannot be formed from the rhumb line by projection on a plane at right angles to the axis of projection. All the others can. See also Hymers's Geometry of Three Dimensions, p. 136.

We shall take a few particular cases for illustration.

1*mo*, Let $e = 0$; that is, let the projection become the *plane of the equator*.

Then (a). Let the projecting point M coincide with c, or the projection be *gnomonic*; we shall have, in this case,

$$\frac{1}{v} = \pm \frac{a k^{-\theta} - a k^{\theta}}{2a \times o}, \text{ or } \quad \frac{1}{v} = \pm \text{inf. or } v = 0. \quad \ldots\ldots\ldots \quad (3.)$$

agreeing with what we know ought to take place, as the only intersection of the projecting cone with the plane of projection, is now the centre itself, indicated by $v = 0$, for all values of θ.

(b). Let M coincide with B; then $b = a$, and we have

$$\frac{1}{v} = \pm \frac{2ak^{-\theta}}{2a^2}, \text{ or } v = \pm a k^{\theta} \quad \ldots\ldots\ldots \quad (4.)$$

which is the equation of the *logarithmic spiral*, agreeing with the well known result of Dr Halley *. and of James Bernoulli †.

(c). Let M be at A; then $b = -a$, and the formula becomes

$$\frac{1}{v} = \mp \frac{2ak^{\theta}}{2a^2}, \text{ or } v = \mp a k^{-\theta} \quad \ldots\ldots\ldots \quad (5.)$$

The reversed branch of the logarithmic spiral.

(d). Let b = infinity. Then

$$\frac{1}{v} = \pm \tfrac{1}{2} \cdot \frac{k^{-\theta} + k^{\theta}}{a} \quad \ldots\ldots\ldots \quad (6.)$$

which is the equation of the *orthographic projection* of the rhumb-line on the plane of the equator. It is independent of the value of c, that is, the projection is the same through whatever point of the axis the parallel to the equator is drawn, as it ought to be.

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* Phil. Trans. 1696. Abt. vol. i. p. 577., or New Abt. vol. iv. p. 68.

† Jac. Bern. Op. Omn. Ed. Cramer, tom. ii. p. 491.

2do, Let $c = a$; then the plane of projection is the upper polar tangent plane, the same as used by JORDANUS NEMORARIUS in his Construction of the Planisphere*.

(a). Let $b = 0$, or the gnomonic be taken. Its equation is,

$$\frac{1}{v} = \pm \frac{1 - k^{2t}}{2\,a\,k^t} = \pm \frac{k^{-t} - k^t}{2\,a} \quad \text{.........} \quad (7.)$$

(b). Let $b = a$, then we have the *stereographic of* JORDANUS, and its equation is,

$$\frac{1}{v} = \pm \frac{2\,ak^{-t}}{4\,a^2}, \text{ or } v = \pm 2\,ak^t \quad \text{.........} \quad (8.)$$

where v is, as it should be, double its former value.

(c). Let $b = -a$, then we have the Jordanian stereographic of the reversed branch of the logarithmic spiral, viz.

$$v = \mp 2\,a\,k^{-t} \quad \text{............} \quad (9.)$$

(d). Let $b =$ infinity, then a vanishes in comparison of b, and we have again the equation of the orthographic projection of the loxodrome, viz.

$$\frac{1}{v} = \pm \frac{k^{-t} + k^t}{2\,a}, \text{ as found in (6.)}$$

3tio, Let $c = -a$, then the projection is on the *lower* polar tangent plane, and we shall have in a similar manner, when we put

(a) . . . $b = 0$, the *gnomonic*, or

$$\frac{1}{v} = \mp \frac{k - 0 - k^t}{2\,a} \quad \text{.........} \quad (10.)$$

(b) . . . $b = a$, then

$$\frac{1}{v} = \pm \frac{2\,a\,k^{-t}}{2\,a \times 0}, \text{ or } v = 0 \quad \text{.........} \quad (11.)$$

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* See the two editions of this tract, Basle, 1536, p. 280, edited by ZIEGLER, and the other edited by COMMANDINE, Ven. 1558, p. 30. I have given some account of these in the Mathematical Repository, vol. vi. pt. ii. p. 42, in treating of the History of the Stereographic Projection of the Sphere.

the equation of the point of projection. The same remark applies as is made on equation (3).

(*c*). $b = -\dot{a}$, the reversed Jordanian branch of the curve, denoted by

$$\frac{1}{v} = \mp\frac{k^{t}}{2a}, \text{ or } v = \mp 2ak^{-t} \quad \ldots\ldots\ldots \quad (12.)$$

(*d*). b = inf. Then, as before the orthographic becomes

$$\frac{1}{v} = \pm\frac{k^{-t}+k^{t}}{2a}.$$

XXXIV.

Mercator's Projection—or, more properly, *Wright's Development*—will next present itself to our consideration.

The following process, for all cases of *functional projection* * on an equatorial cylinder, will furnish the relation between the latitude and its projection, if we admit the possibility of resolving the equations.

$$\text{Let} \quad x = fy \ldots\ldots\ldots\ldots\ldots\ldots\ldots (1.)$$
$$\theta = f_{\prime}\phi \ldots\ldots\ldots\ldots\ldots\ldots\ldots (2.)$$
$$\frac{x}{r} = \theta \ldots\ldots\ldots\ldots\ldots\ldots\ldots (3.)$$
$$y = \mathrm{F}\phi \ldots\ldots\ldots\ldots\ldots\ldots\ldots (4.)$$

The first of these is the equation of the development of the functional projection † on the equatorial cylinder, when that cylindrical surface is unrolled

* See note (D).

† This very appropriate epithet seems to have been introduced into the science by Mr Gompertz.—See his second tract on Imaginary Quantities, p. x. Some authors have been so far from understanding the nature of "Mercator's Projection," that they have designated it as made "*upon a plane at an infinite distance*"!—Dealtry's Fluxions, p. 427. This singular oversight originated, doubtless, in the want of a proper verbal distinction being made between the radial projection of a figure upon a given surface, and the figure generated upon that surface, by taking as co-ordinates some *function* of the curve said to be "projected."

upon a plane. The second is the equation of the curve traced on the spherical surface. The third simply expresses that the equator becomes the axis of x in the development; and the fourth is the relation between the radius-vector of the curve at that point, and the said functional projection of its complement on the cylinder.

Equation (3.) is always given; and most commonly (1.) and (2.) are given to find (4.). This is the case we propose at present. To effect it,

Put (1.) in (3.), and equate the result to (2.) This gives

$$\frac{fy}{r} = f_{\prime}\phi; \text{ or } fy = rf_{\prime}\phi;$$

$$\therefore y = f^{-1} rf_{\prime}\phi \ldots\ldots\ldots\ldots\ldots\ldots\ldots (5.)$$

Applying this to find the law of projection which shall so divide the meridian as to represent the loxodrome by a straight line on the developed cylinder, we have

$$x \cos a + y \sin a = 0; \text{ or } x = -\tan a \,.\, y \ldots\ldots\ldots\ldots\ldots (6.)$$

$$\theta \cot a = \log \tan \tfrac{1}{2}\phi; \text{ or } \theta = \tan a \log \tan \tfrac{1}{2}\phi \ldots\ldots\ldots (7.)$$

The former of these is the equation of the straight line cutting the prime meridian under an angle a, and lying in the $(+\phi, -\theta)$ region of the sphere. The second is the equation of the loxodrome (XXXI. 4.) resolved for θ.

Inserting these in (5.) we have

$$\begin{aligned} y = f^{-1} rf_{\prime}\phi &= -\cot a\, r \tan a \log \tan \tfrac{1}{2}\phi \\ &= -r\, l \tan \tfrac{1}{2}\phi; \text{ or} \\ -y &= rl \tan \tfrac{1}{2}\phi. \end{aligned}$$

Hence,
$$\begin{aligned} \epsilon^{-\frac{y}{r}} &= \tan \tfrac{1}{2}\phi, \text{ and} \\ \epsilon^{\frac{y}{r}} &= \cot \tfrac{1}{2}\phi, \text{ or} \\ y &= rl \cot \tfrac{1}{2}\phi, \ldots\ldots\ldots\ldots\ldots\ldots\ldots (8.) \end{aligned}$$

which is the usual formula given for this purpose, and which is the solution of equation (4.) above given.

Though objections have often been made to this chart, for alleged inaccuracy of principles, yet, with two exceptions, they are undeserving of

notice, as resulting from the total ignorance of the objectors. The two cases I refer to, however, were the result of mistakes on the part of men of respectable mathematical attainments for the times they lived, and the opportunities of cultivating this branch of study which they possessed. They were Mr Henry Wilson, in his proposal for Curvilinear Sea Charts, 1720; and the Rev. Mr West of Exeter, in his Posthumous Works, 1762, published by the Rev. Mr Rowe. Of Mr Wilson's book I can only speak from the report of others; but Mr West's is certainly indicative of a degree of mathematical acquirement, that shews the error was the result of haste rather than incapability of investigation. He was led, by trusting too implicitly to the unguarded *verbal statements* of Wright, to suppose, that if the loxodrome were gnomonically projected upon the equatorial cylinder, and this cylinder unrolled upon a plane, then this projection would become a straight line. The error was pointed out by Mr Samuel Dunn, in a letter to the Royal Society; and this letter, and the report made upon it by the Rev. William Mountaine (to whom the letter had been referred by the Council of the Royal Society), were printed in the Philosophical Transactions for 1763 *. It was also noticed by Mr George Witchell, who also determined the equation of the curve which would be actually formed in that case, in the Ladies' Diary for 1764 †. I have no intention of entering anew upon this argument, and I allude to it only as it affords me another opportunity of employing the method of examining the relations between curves and their projections upon the sphere, and its equatorial cylinder; and that the cases before us afford a good illustration of that very simple method.

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* Page 66. and 69.

† Vide Professor Leybourn's edition of this curious, and in many respects valuable, Miscellany, vol. ii. p. 249. We might, in a manner very similar to that just employed, determine the development of the Stereographic projection of the Loxodrome upon the equatorial cylinder, and should find, as is done in the Gentleman's Diary for 1820, p. 43. that it is the logarithmic curve.

To find the equation of the developed gnomonic projection of the loxodrome.

Here, we have $\cot\phi = \pm\frac{1}{2}\cdot\{\kappa^{-\theta} - \kappa^{\theta}\}$, from (XXXI. 7.)

$y = r\cot\phi$,...the condition of gnomonic projection;

$\frac{x}{r} = \theta$,...the condition of equatorial contact.

From which $f_{\prime\prime}(x, y) = 0$ is readily found to be

$$y = \pm\frac{r}{2}\{\kappa^{-\theta} - \kappa^{\theta}\},\ldots\ldots\ldots\ldots(9.)$$

Again, *To ascertain what curve on the sphere, gnomonically projected on the equatorial cylinder, would give a straight line upon the development.*

Our equations are now

$$y = r\cot\phi,$$

$$\frac{n}{r} = \phi,$$

$$n\cos a + y\sin a = 0.$$

From which we get at once the equation sought,

$$\left.\begin{array}{l}\theta = -\cot\phi\tan a, \text{ or}\\ \cot\phi = -\cot a\,.\,\theta\end{array}\right\}\ldots\ldots\ldots\ldots(10.)$$

It is obvious, however, that our inquiries in this way, though they may often lead to interesting results in particular cases, are, analytically speaking, exceedingly confined, on account of our confined power of solving equations. To pursue it further here, would therefore be irrelevant.

XXXV.

The length of the loxodrome has been already often assigned; and, as before observed, by methods which approximate very closely to our own.

The expression

$$\pm\sqrt{\sin^2\phi\, d\theta^2 + d\phi^2}$$

becomes, on account of (XXXI. 2.),

$$\pm d\phi \sec a \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(1.)$$

and hence the length of the arc between the limits ϕ' and ϕ'', is

$$L = \pm(\phi' - \phi'') \sec a \ldots\ldots\ldots\ldots(2.)$$

agreeing in all respects with the commonly obtained result.

The area:—this gives

$$dA = (1 - \cos\phi)\, d\theta = \frac{1 - \cos\phi}{\sin\phi}\, d\phi \,.\, \tan a \ldots\ldots\ldots\ldots(3.)$$

or integrating,

$$A = 2 \log\cdot\sec\frac{\phi}{2} \,.\, \tan a \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4.)$$

When $\phi = 0$, $\sec\frac{\phi}{2} = 1$, and $\log\sec\frac{\phi}{2} = 0$. Hence, if we suppose the area to commence from the pole, and the radius-vector to revolve in the positive direction, the whole surface traced out during the first quadrant of of ϕ, will be $= \tan a \log 2$.

XXXVI.

Given the base and vertical angle of a spherical triangle, to find the locus of its vertex.

Let $a_{,}\, \beta_{,}$ and $a_{,,}\, \beta_{,,}$ be the co-ordinates of the extremities of the base, and $\phi\theta$ those of the vertex. Then, if we denote by δ, $\delta_{,}$, $\delta_{,,}$ the three sides of any one of these triangles, and the given angle by ϵ, we shall have (by II.) for the determination of the locus, the four following equations:

$$\cos\delta = \cos a_{,} \cos a_{,,} + \sin a_{,} \sin a_{,,} \cos\overline{\beta_{,} - \beta_{,,}} \ldots\ldots\ldots(1.)$$

$$\cos\delta_{,} = \cos\phi \cos a_{,,} + \sin\phi \sin a_{,,} \cos\overline{\theta - \beta_{,,}} \ldots\ldots\ldots(2.)$$

$$\cos\delta_{,,} = \cos\phi \cos a_{,} + \sin\phi \sin a_{,} \cos\overline{\theta - \beta_{,}} \ldots\ldots\ldots(3.)$$

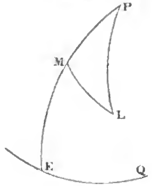
$$\cos\delta = \cos\delta_{,} \cos\delta_{,,} + \sin\delta_{,} \sin\delta_{,,} \cos\epsilon \ldots\ldots\ldots\ldots(4.)$$

Inserting in (4.) the values of $\delta\, \delta_{,}\, \delta_{,,}$ from (1, 2, 3.), we shall have the

equation sought: but except in the case of $\epsilon = \frac{\pi}{2}$, the reduction presents a result too complex to enable us to judge of the nature of the curve. We shall, therefore, for this purpose, take particular values of constants; that is, instead of taking the origin and direction of co-ordinates arbitrarily, we shall confine them to specific positions. In cases where the intersection of such a locus, arbitrarily situated with respect to other given curves, is sought, we must of course recur to the original equations above given.

1*mo*, Let the pole of reference be the extremity of the base.

Fig. 27.



Put $PM = \lambda$, and take the meridian MP as origin of θ. Then, $PL = \phi$. Let the given angle $MPL = L$; then, by a known theorem in spherical trigonometry, we have at once the equation

$$\cot \lambda \sin \phi - \cot L \sin \theta = \cos \phi \cos \theta \quad \text{.......... (5.)}$$

Resolving this with respect to ϕ, we obtain

$$\cos = -\frac{\cot L \sin \theta \cos \theta \pm \cot \lambda \{\operatorname{cosec}^2 \lambda - \operatorname{cosec}^2 L \sin^2 \theta\}^{\frac{1}{2}}}{\operatorname{cosec}^2 \lambda - \sin^2 \theta} \quad \text{.......... (6.)}$$

$$\sin \phi = +\frac{\cot L \cot \lambda \sin \theta \mp \cos \theta \{\operatorname{cosec}^2 \lambda - \operatorname{cosec}^2 L \sin^2 \theta\}^{\frac{1}{2}}}{\operatorname{cosec}^2 \lambda - \sin^2 \theta} \quad \text{.......... (7.)}$$

2*do*. Taking it now in reference to an origin at the middle of the base, and still as a polar curve, we have, putting $RP^* = PT = a$ (the same as $\frac{1}{2}\lambda$

* See Figure on page 350.

in the last case); $PM = \phi$, $RPM = \theta$, and $RMT = L$, we shall have

$$\cot^{-1}\frac{\cot a \sin\phi - \cos\phi\cos\theta}{\sin\theta} + \cot^{-1}\frac{\cot a\sin\phi + \cos\phi\cos\theta}{\sin\theta} = L \ldots\ldots\ldots (8.)$$

or, taking cotangents and reducing, we get

$$\frac{\{\operatorname{cosec}^2 a - \sin^2\theta\}\sin^2\phi - 1}{2\cot a\sin\phi\sin\theta} = \cot L \ldots\ldots\ldots (9.)$$

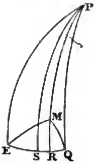
or, resolved for ϕ, this gives

$$\sin\phi = \frac{\cot L\cot a\sin\theta \pm \{\operatorname{cosec}^2 a - \overline{1 - \cot^2 a\cot^2 L}\,.\sin^2\theta\}^{\frac{1}{2}}}{\operatorname{cosec}^2 a - \sin^2\theta} \ldots\ldots\ldots (10.)$$

3tio, Take the same origin of ϕ, but refer θ to a meridian PQ at right angles to RT, then, we have merely to interchange $\cos\theta$, $\sin\theta$, and we get

$$\sin\phi = \frac{\cot L\cot a\cos\theta \pm \{\operatorname{cosec}^2 a - \overline{1 - \cot^2 a\cot^2 L}\,.\cos^2\theta\}^{\frac{1}{2}}}{\operatorname{cosec}^2 a - \cos^2\theta} \ldots\ldots\ldots (11.)$$

4to, If, on the contrary, we take rectangular co-ordinates, P being still the origin, we shall have



$$L = \cot^{-1}\cot\overline{a+\theta}\sin\phi + \cot^{-1}\cot\overline{a-\theta}\sin\phi \ldots\ldots\ldots (12.)$$

or, taking the co-tangents of both sides,

$$\frac{\sin^2\phi\cot\overline{a+\theta}\cot\overline{a-\theta} - 1}{\sin\phi\,(\cot\overline{a+\theta} + \cot\overline{a-\theta})} = \cot L \ldots\ldots\ldots (13.)$$

and, by putting for these co-tangents, their values, in terms of the sines, &c. it is

$$\sin\phi = \frac{\cot L\sin 2a \pm \{\cot^2 L\sin^2 2a - \sin^2 2\theta\}^{\frac{1}{2}}}{\cos 2\theta - \cos 2a} \ldots\ldots\ldots (14.)$$

XXXVII.

We proceed now to consider a few circumstances connected with this curve. The first form (5.) possesses that kind of analytical symmetry which may claim for it some attention; but it is of more importance, from its being *the geographical equation of the lines of equal magnetic variation*, the needle being supposed constantly directed to the magnetic pole. In this case, λ is the colatitude of the magnetic pole, L is the magnetic variation, ϕ the colatitude of any point in the curve, and θ is the corresponding longitude, reckoned from the meridian in which the magnetic pole is situated. If another meridian be taken as the origin of longitudes, such that, in reference to it, the longitude of the magnetic pole is l; then, the equivariation-curve is represented by the equation

$$\cot \lambda \sin^2 \phi - \cot \mathrm{L} \sin (\theta - l) = \cos \phi \cos (\theta - l) \ldots \ldots \ldots (15.)$$

The beautiful experiment of Mr Barlow, which renders it highly probable that the phenomena of terrestrial magnetism result from thermo-electric causes, confers great philosophical interest upon this equation. The resemblance of the lines along which the compass was carried in that experiment to obtain a variation of L degrees, to the lines of equal variation furnished by the observations of Halley, and his successors in the inquiry, suggested that if we could assign this hypothetic path, in the form of an equation referred to our common geographical co-ordinates, we should greatly facilitate such comparisons as may be made between the phenomena and the results of any presumed law of physical action by which that direction was given to the needle. Independently, too, of this purpose, we should be able, by means of that equation, to compare these hypothetical curves with the curves furnished by observation, and thus (the general resemblance being admitted) find what deviations from these upon the surface of the earth, arising from disturbing causes, or from "local attractions." But this is irrelevant.

The character of these curves is best obtained from equation (10.), and we proceed to examine it.

1*mo*, Let $\sin \theta = 0 \;\therefore\; \theta = n\pi$, and $\sin \phi = \pm \sin a$

$\therefore\; \theta = 0$, or $\theta = \pi$, and $\phi = +a$, or $\phi = -a$:

or the curve passes through R and T; and through R′ and T′, points equidistant from the opposite pole P′.

2do, Let $\sin\theta = 1$: then, $\theta = \frac{4n+1}{2}\pi$,

and the equation becomes

$$\sin\phi = \frac{\cot L \cot a \pm \{\operatorname{cosec}^2 a - 1 + \cot^2 a \cot L\}^{\frac{1}{2}}}{\operatorname{cosec}^2 a - 1} =$$

$$= \frac{\cot L \pm \operatorname{cosec} L}{\cot a} = \frac{\sin a\,(\cos L \pm 1)}{\sin L \cos a} \text{..............(16.)}$$

This gives two values of $\sin\phi$, viz.

$$\sin\phi = \frac{2\sin a \cos^2 \frac{1}{2} L}{2\sin\frac{1}{2}L\cos\frac{1}{2}L\cos a} = \cot\tfrac{1}{2}L\tan a \text{..............(17.)}$$

$$\sin\phi = -\frac{2\sin a \sin^2 \frac{1}{2} L}{2\sin\frac{1}{2}L\cos\frac{1}{2}L\cos a} = -\tan\tfrac{1}{2}L\tan a \text{........(18.)}$$

3tio, Let $\sin\theta = -1$, or $\theta = \frac{4n-1}{2}\pi$;

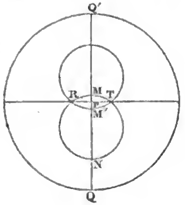
then the equation becomes

$$\sin\phi = -\frac{\cot L \cot a \pm \cot a \operatorname{cosec} L}{\operatorname{cosec}^2 a - 1} = \frac{-\cot L \pm \operatorname{cosec} L}{\cot a} = \frac{\sin a\,(\cos L \mp 1)}{\sin L \cos a} \text{...(19.)}$$

This divides itself into

$$\sin\phi = -\frac{2\sin a\cos^2\frac{1}{2}L}{2\cos a\sin\frac{1}{2}L\cos\frac{1}{2}L} = -\cot\tfrac{1}{2}L\tan a \text{.........(20.)}$$

$$\sin\phi = \frac{2\sin a\sin^2\frac{1}{2}L}{2\sin\frac{1}{2}L\cos\frac{1}{2}L\cos a} = \tan\tfrac{1}{2}L\tan a \text{..............(21.)}$$



There are hence on each side of the circle RT, two branches of the curve, which are two and two symmetrical. They intersect PQ PQ′ in points MN, M′N′; so that PM = PM′, and PN = PN′. To ascertain whether the symmetrical branches are likewise continuous or not, we have only to consider that the *same* value of θ gives *unequal* values of sin ϕ, and hence the two continuous pairs of branches are not the symmetrical ones. The curve, then, is composed of two separate ovals, which intersect each other in R and T; but they are at the same time in all respects equal, and only situated in a reversed position, with respect to each other and RT, as in the annexed figure. The two unequal branches into which RT divides each oval, are the loci of the given angle, and of its supplement; as is obvious from the formula determining the locus by means of the *sine* of the vertical angle, that sine being also the sine of the supplement of the given angle. The same thing takes place when the problem is solved *in plano*: the locus by these two circles very much resembling our figure above in general appearance, and having several analogous properties.

Besides these, there are two others, which constitute the locus of the secondary intersections of the circles RM, TM, which contain the given angle, and surrounding the opposite pole. The secondary branch of RNP is *conically** opposite to its primary, and *cylindrically* opposite to the primary branch of RN″T: and, *vice versa*, the secondary branch of RN″T is *conically* opposite to its primary, and *cylindrically* to the primary branch of RNT. The primary and secondary branches of each individual (both of RNT and RN″T) are obliquely cylindrical to each other: and so are the two primary branches themselves, as well as their secondaries, obliquely cylindrical.

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* Vide Note B.

NOTES.

NOTE A.

It will often facilitate our detection of circular loci, to frame a table of solutions of (I. 1.), with respect to each of the functions of the arcs employed. The following is a specimen:

Less Circle to radius ϱ. Great Circle, or $\varrho = \frac{\pi}{2}$.

$$\sin\phi = \frac{\cos\varrho \sin\lambda \cos\overline{\theta - \varkappa} \pm \cos\lambda \sqrt{\sin^2\varrho - \sin^2\lambda \sin^2\overline{\theta - \varkappa}}}{1 - \sin^2\lambda \sin^2\overline{\theta - \varkappa}} = \frac{\pm \cot\lambda}{\sqrt{1 - \sin^2\lambda \sin^2\overline{\theta - \varkappa}}} = \frac{\pm\cot\lambda}{\sqrt{\cot^2\lambda + \cos^2\overline{\theta - \varkappa}}}$$

$$\cos\phi = \frac{\cos\varrho \cos\lambda \pm \sin\lambda \cos\overline{\theta - \varkappa}\sqrt{\sin^2\varrho - \sin^2\lambda \sin^2\overline{\theta - \varkappa}}}{1 - \sin^2\lambda \sin^2\overline{\theta - \varkappa}} = \frac{\pm\cos\overline{\theta - \varkappa}\sin\lambda}{\sqrt{1 - \sin^2\lambda \sin^2\overline{\theta - \varkappa}}} = \frac{\pm\cos\overline{\theta - \varkappa}}{\sqrt{\cot^2\lambda + \cos^2\overline{\theta - \varkappa}}}$$

$$\cot\phi = \frac{\cos\varrho\cos\lambda \pm \sin\lambda\cos\overline{\theta - \varkappa}\sqrt{\sin^2\varrho - \sin^2\lambda\sin^2\overline{\theta - \varkappa}}}{\cos\varrho\sin\lambda\cos\overline{\theta - \varkappa} \pm \cos\lambda\sqrt{\sin^2\varrho - \sin^2\lambda\sin^2\overline{\theta - \varkappa}}} = \pm\frac{\cos\overline{\theta - \varkappa}}{\cot\lambda} = \pm\tan\lambda . \cos\overline{\theta - \varkappa}$$

The tangent, secant, and cosecant, being the reciprocals of these, may be formed by inspection, and need not be tabulated. We might have inserted the other functions of θ, or we might have resolved the primary equation in respect of θ, and in terms of each function of ϕ; but the process is too simple and elementary to need more than pointing out to the reader.

NOTE B.

It becomes of great importance to form an analytical table of the significations of spherical equations; that is, for determining the points signified by $f(\phi\theta) = 0$, resolved for either of the variables. We propose to furnish one in this note.

All trigonometrical functions belong to more than one arc—to innumerable arcs. So long, however, as we attend only to the points in which those arcs terminate, these points are, for each function, confined to two, the successive pairs terminating at the same points after the increment of an indefinite number of complete circum-

ferences. So far, then, as the geometrical position of a point on the sphere, each function determines *two points*, and *only two*.

The sphere is divisible into eight octants, each bounded by three quadrantal arcs. A point may be situated any where upon any one of these, or upon any one of their bounding quadrants. To arrive at that point, we may go two ways from the first meridian, and two ways from the pole; and hence any one point may be denoted by four different pairs of co-ordinates. For facilitating the description, we shall suppose a common terrestrial globe, having its meridian in the plane of the terrestrial meridian, and its equator in the horizon. Its north pole will then be P, its south pole P′, and its equator will be EQ; and if London be brought to the brass meridian, the longitudes on this globe will correspond with the values of θ on our sphere, whilst φ will correspond with the co-latitude (or rather say N. P. D.) of the point under consideration. Let us now conceive the globe to be permanently fixed in its present position, but the brass meridian to revolve about the axis, either eastward or westward; and we shall be able to comprehend, with the utmost facility, the distribution of the co-ordinates φ, θ into the four pairs above mentioned.

In analogy to the method of estimating rectilinear co-ordinates, we shall consider (the sphere being fixed, as already described) that the values of θ lying towards the right hand, or east longitude, is +, and that to the left, or west, is —. We shall also, when we take the *polar* equation, consider the radius-vector φ, which coincides with the prime meridian on the south side, to be +, and the opposite one to be —. These can never change, except by becoming respectively $= 0$. We shall designate the eight spherical octants by "*regions*;" and we shall describe the distances from the origin of θ by calling it the first, second, third, &c. quadrant of *positive revolution*, or of *negative revolution*. The same method will be well adapted with respect to *positive and negative polar distance*, in polar curves, or to *positive and negative latitude*, in rectangular curves.

If, therefore, the co-ordinates of a point on the sphere be

$$\varphi,\ \theta, \ldots\ldots\ldots\ldots\ldots\ldots(1.)$$

they are also

$$\varphi,\ -(2\pi-\theta), \ldots\ldots\ldots\ldots\ldots\ldots(2.)$$
$$-(2\pi-\varphi),\ -(2\pi-\theta) \ldots\ldots\ldots(3.)$$
$$-(2\pi-\varphi),\ \theta \ldots\ldots\ldots\ldots\ldots\ldots(4.)$$

respectively. This can hence create no difficulty.

Again, when we have resolved the equation $f(\varphi, \theta) = 0$ with respect to a *function* of one of the variables, we shall always find two points belonging to each value of that variable on the sphere, for every individual value of the other variable. Now, as in general the equations between these variables in trigonometrical functions, and as to each value of any such function two arcs belong, we shall, (supposing only one function of each variable appear in the final equation) obtain *four points on the sphere*. To

find, then, the number of points which are furnished of the locus—or, in other words, of how many branches the locus may at the most consist—we must resolve all the functions of ϕ into one particular function (no matter which) of ϕ, and all the functions of θ into any particular function of θ. If the degree of this polynome be n, then the number of points furnished by each value of the trigonometrical function which is taken as the principal variable, will determine *four* points on the surface of the sphere. The curve *may*, then, be composed of $4n$ branches: at least in an analytical point of view, such will be the case. If, however, we seek the true *geometrical* number, we shall find it to be $2n$, as each of the pairs which result from this resolution, are reduplications of the other pair, each of each. Either branch may be described by a diameter of the sphere moving upon the other branch, and leaving upon the sphere the trace of its other extremity. This second branch is, indeed, the locus of the second intersection of the circles whose primary ones trace out the primary branch of the locus, when the locus is that of the intersections of two circles, described according to an assigned law; and, in all cases, there is some circumstance in the signification of the problem correspondent to the second intersection. We have only to lay down the relations of these branches, as they depend upon the functions by which they are determined.

1*mo*, Let it be resolved for ϕ. Then taking the *polar* equation,

$$\sin\phi = f(\theta) \quad \ldots\ldots\ldots\ldots \quad (5.)$$

This denotes two points upon the same meridian, equidistant from the poles, and the locus itself surrounding both poles, is composed of branches perfectly equal and similar in all respects, except that these two branches are also upon a right cylinder, which is perpendicular to the equator. The primary and secondary branches are therefore *cylindrically* related to each other.

Next, take $$\cos\phi = f(\theta) \quad \ldots\ldots\ldots\ldots \quad (6.)$$

This gives two branches, lying opposite ways upon the radius vector ϕ; these *may* become coincident; they *do so* when the value of $\cos\phi$ is single, and marked $+$ or $-$.

Again, take $$\tan\phi = f(\theta) \quad \ldots\ldots\ldots\ldots \quad (7.)$$

Now, since $\tan\phi = \tan(\pi + \phi)$, there are two points belonging to this diametrically opposite to one another upon the sphere, each of which may be considered to trace a branch round either pole. These, as before, may become coincident. The cotangent also being the reciprocal of the tangent, the same circumstance takes place. The equation of the hour-lines on the Antique Dials is an instance of this. If we consider the polar equation of these curves, viz. $\cot\phi = +\tan\lambda\cos n\,\theta$,

we shall find a perfect correspondence between this result and that given in the paper on that subject. The two branches are here *conically opposite.*

In case of secant and cosecant of ϕ, we have the same class of results as those afforded by the cosines and sines, of which they are the reciprocals.

2do, Resolve the equation for ϕ: and take it as a rectangular equation. We shall have simply an interchange of sines with cosines, tangents with cotangents, and secants with cosecants. The *sines* will now give cylindrically opposed curves, and cosines reduplicating curves: tangents and cotangents as before: and secants and cosecants give results similar in that respect with the cosines and sines.

3tio, Resolve it for θ: it will now be more convenient, generally, to consider the curve as referred to rectangular co-ordinates.

The sines will give curves cylindrically opposite, and referred to the prime meridian and its opposite branch. The cosines will give curves symmetrically related to prime meridian, lying to the right and left of it. The tangents and cotangents will give curves of branches diametrically opposite; and the secants and cosecants, the same classes as those furnished by cosines and sines.

NOTE C.

Though I have limited myself to a mere indication of the general character of the spherical conic sections, it may not be irrelevant to state here, that all the properties of contact and intersection, all the properties of inscribed and circumscribed polygons, and most of the loci generated from these curves, by given methods, when the figures are taken *in plano,* have analogues on the surface of the sphere. The circle, however, which in the plane conic sections becomes a locus in consequence of containing a given angle subtended by a base given in magnitude and position, is exchanged *in sphæro* for the curve discussed in XXXVI and XXXVII: but this change does not take place when the locus is the path of a given angle contained by circles described by any other law. The perpendicular from the focus upon the tangent is an instance of this latter kind. *In plano* and *in sphæro,* it is alike a circle on the major axis. The various divisions of great circles any way related to the defining data, or to lines definitely drawn with respect to the fixed lines and fixed points of the locus, have analogues in terms of some of their trigonometrical functions to the sections of the corresponding straight lines *in plano.* A few of

these latter will be investigated in a paper which will be printed in the next number (XXIII) of Professor Leybourn's *Mathematical Repository*.

I have examined a great number of the loci thus produced, and in no case that I have yet tried have I found the analogy broken. A few of the more curious, elegant, and important ones will be inserted in the succeeding portion of this inquiry, which I am preparing to send to the Royal Society of Edinburgh. In that paper I shall examine those properties of spherical curves whose demonstrations are dependent upon the differential co-efficients, or most readily investigated by means of them,—as their *singular points, asymptotic circles, asymptotism in general, their spherical involutes and evolutes*, and other topics allied to these. I shall then enter into a full examination of several paradoxical expressions that have occurred to me in my researches, arising out of the application of general formulæ to particular cases: as, for instance, in the *expression of the area of spherical curves taken between specified limits* (see Art. XIX. of this paper), and other collateral subjects. Two other topics will also claim a little consideration—*oblique spherical co-ordinates, and the locus of the penetration of the sphere by any given surface, but especially by a concentric cone of the second degree.*

I may just remark, in addition to the little that is said in the article to which this note alludes (XXIV), that, taking the focus for origin of ϕ and the line joining the foci for origin of θ, the general equation of the conic sections on the sphere is

$$\tan\phi = \frac{\cos 2\alpha - \cos 2\iota}{\sin 2\alpha \pm \sin 2\iota \cos\theta}$$

where 2ι is the distance of the foci, and 2α is the given sum or difference of the focal radii vectores drawn to the points of the locus. This, for the sum, is, abating the notation, the same as Professor Lowry's equation of the spherical ellipse, in the old series of Leybourn's Repository, p. 196. See Note (E).

In the case of the spherical parabola, $2\alpha = \frac{\pi}{2}$, and the expressions are for this case

$$\tan\phi = \frac{\cos 2\iota}{1 \pm \cos\theta}.$$

The analogy between these and the focal polar equations of the plane conic sections, is sufficiently remarkable to interest every inquiring mind.

NOTE D.

In Hymers's Analytical Geometry of Three Dimensions, p. 136, *the orthographic projection of the rhumb-line on the plane of the equator is stated to be the hyperbolic spiral.* A moment's consideration will shew that this is a mistake. It is this: *The projection of the spherical surface itself upon the plane of the equator is confined to the limits of the circle of the equator; and hence the orthographic projection of any curve traced upon that spherical surface must also be confined to those same limits: but the hyperbolic spiral has a rectilinear asymptote, and hence this curve is not confined within the limits assigned above. The projection, therefore, whatever it might be, is not the hyperbolic spiral.* The mistake was most probably an oversight in writing the *name* of the spiral, the projection being really one of Cotes's spirals, or at least a case of one of them.

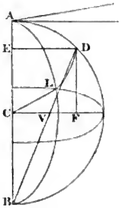
Dr Lardner also, in his valuable treatise on Algebraic Geometry, has made an oversight concerning the projections of this curve. See p. 478. He says, "If a logarithmic spiral be described upon the plane of a great circle of the sphere, with the centre as a pole, and *perpendiculars be drawn from every point in it to meet the surface of the sphere, the extremities of those perpendiculars will trace out a loxodrome curve, or, in other words, the projection of the loxodrome curve on the plane of the equator is the logarithmic spiral.*"

We have seen that the equation of the *stereographic* projection of the Rhumb-line is the logarithmic spiral (XXXIII. *b*, *c*, &c.); and we have seen (XXXIII. *d*) that the *orthographic* is defined by a different equation, and therefore is a different curve. The equation of the hyperbolic spiral, too, is $v = a\theta^{-1}$, which is a very different expression from that we have found in (*d*). Hence the equation of the hyperbolic spiral does not, as Mr Hymers inadvertently supposed, coincide with that of the orthographic projection of the loxodrome. These curves are not therefore identical. It remains to ascertain the equation of the spherical curve formed by the perpendiculars raised upon the points of the logarithmic spiral for comparison with Dr Lardner's other statement—that it is the loxodrome. We shall also, *en passant*, show the converse of (XXXIII. *b*, *c*); viz. that the stereographic projection of the logarithmic spiral upon a concentric sphere, from one of the poles of the plane of the spiral, is the loxodrome.

1st, *The stereographic projection of the logarithmic spiral upon the surface of a concentric sphere is the loxodrome.*

Let $AC = CB = CL = a$, be the unit of radii-vectores, and CL the origin of polar angles. Join DB cutting CP in V. Denote the log.-spiral by

$$r = k^{\theta} \quad .... \quad (1.)$$



Let $LCV = \theta$, and describe the meridian ALB. Then LAD is also $= \theta$. We have now to find $\widehat{AD}$.

$$\frac{CV}{CB} = \frac{r}{a} = \tan CBV = \tan ABD \ldots\ldots\ldots\ldots\ldots\ldots(2.)$$

But because the angle ABD is at the circumference, the arc AD measures twice the angle ABD: that is, in symbolic language,

$$\widehat{AD} = 2 \tan^{-1} \frac{r}{a} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(3.)$$

$$CF = ED = a \sin \widehat{AD} = a \sin \phi.$$

Hence $\quad a \sin \phi = a \sin 2 \tan^{-1} \frac{r}{a}.$

But $\quad \sin 2 \tan^{-1} \frac{r}{a} = \sin 2 \tan^{-1} \frac{k'}{a}$ by equation (1)

or, taking $a = 1 =$ rad. of sphere, we have

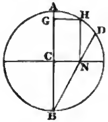
$$\sin \phi = \sin 2 \tan^{-1} k' = 2 \sin \tan^{-1} k' \times \cos \tan^{-1} k',$$

or, by reduction,

$$\sin \phi = \pm \frac{2k'}{1 + k'^2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(4.)$$

which is the equation of the loxodrome, already found in (XXXI, 6), in terms of the variables ϕ, θ. Hence, &c.

We have yet to ascertain the spherical equation of the intersection of the orthogonal cylinder on a log.-spiral base, with the surface of the sphere.



Let N, taking the place of V in the former figure, be a point in the log. spiral, whose equation is

$$v = k^{\theta} \text{..................................} (1.)$$

Draw NH, HG $\perp$ to CN, CA. Then

$$CN = v = k^{\theta} = GH = \sin \widehat{AH}$$

$$\therefore \sin \phi = k^{\theta} \text{..............................} (2.)$$

This is *not* the equation of the loxodrome; and hence, also, this part of the supposition is erroneous also. The mistake probably arose from a momentary confusion of the two methods of projection—the stereographic and the orthographic.

Note E.

The *first attempt*, in all probability, that was made to assign a spherical curve, by means of its equation between ϕ and θ, was by that very ingenious mathematician Mr James Skene of Aberdeen, and by the late Mr Thomas White of Dumfries. The latter gentleman proposed this question in the *Gentleman's Diary* for 1795, and it was answered by the former in the Diary for 1796. See Davis's Collection of those tracts, vol. iii. p. 258, under that date. I had completed the researches contained in the preceding paper before I was aware of that circumstance, which was obligingly pointed out to me by Professor Lowry of the Royal Military College. The solution of Mr Skene is very similar to the preceding XXXVII, 13: and he shewed also that the orthographic projection of the curve upon the equator (P being the pole) is an ellipse.

In Professor Leybourn's *Mathematical Repository*, O. S. Quest. 130, vol. ii. p. 196, the spherical ellipse is proposed, and a solution by Mr Lowry, inserted in the following number, remarkable, like all the processes of that eminent geometer, for the elegance of its methods, and the simplicity of the results. As a more ample discussion of that curve will appear in the next number of the *Repository*, than I could find room for here, I shall desist from further remark upon it at present.

In Howard's *Spherical Geometry* (1798), too, it is proposed, p. 115, to find the locus of XXXVII; which the author considers a new problem. Had it been so, his investigation did not remove the necessity for a totally new consideration of it. These, with the Spherical Parabola (*Repos.* v. p. 240), and the Hectemoria of my former paper, are, so far as I know, all the attempts that have been made to treat spherical loci by means of spherical co-ordinates. The curve of pursuit, treated by Simpson and Emerson in the *Ladies' Diary* 1736, was considered as a plane curve; and the same is true of Mr Cunliffe's note upon these, in Leybourn's edition of

that work, iv. p. 275. Mr Skene (β. Cygni) gave a differential equation of the same upon the sphere itself, but did not attempt the integration of it, in *Gentleman's Math. Comp.* No. vii. *Vid.* also Leyb. *Diaries*, iv. 277. No general views, however, of extending and systematizing the inquiry seem to have occurred to any of these writers; and perhaps, whilst the old trigonometrical notation was so generally employed, the advantages of the new too little felt, and the formula and methods which were connected—almost identified—with it, imperfectly familiarized to the eye and hand of the analyst;—it would, then, have been almost impossible to pursue those inquiries to any great extent. At all events, the subject died away, and was probably forgotten even by those who had engaged in it at the time. I think it right, however, to recapitulate what has been already done, though at the same time to state distinctly, that my own researches were undertaken and completed in perfect ignorance that even these few attempts had been made before me.

ERRATUM.

Page 269, line 7, *after* vertical *add* angle

END OF PART FIRST.

On the Determination of the Position of Strata in Stratified Rocks. By L. A. Necker, Honorary Professor of Mineralogy and Geology in the Academy of Geneva, &c.

(*Read 6th February* 1832.)

It has always appeared to me, that the study of the stratification of rocks and of mountain masses, ought to be one of the principal objects of a geological observer. Many of the most important facts in geology have been ascertained by the consideration of the position of strata. Among these facts, the relation existing between the direction and the inclination of the strata and the unstratified rocks, to whose presence the change in the position of the beds from an horizontal to an inclined, and sometimes even to a vertical situation, is now generally attributed, is one of the most conspicuous. It is only by an accurate determination of the position of the strata in any mountain-chain that the real direction of the line of elevation of that chain or its mineralogical axis may be determined.

However, such a determination is not always so easily accomplished as it is in mines, in quarries, or in such places where the upper or under surface of a stratum being exposed to the view of the observer, admits of the immediate application of one of the different kinds of clinometers well known to geologists. In high mountain-chains, the real direction and inclination of the strata can only be concluded from the examination of the positions of those lines which the seams of the strata form upon the face of abrupt and precipitous rocks, often inaccessible to the most hardy mountaineer. In such cases, the inexperienced observer, who would be tempted to consider the dip of those lines

or seams, as corresponding to the true inclination of the plane of the strata, would be entirely misled; and if, from this supposed inclination of the plane, he wished to infer the bearing of the strata, he would meet only with the most inextricable confusion, instead of perceiving that remarkable regularity in the direction of the strata which is so conspicuous in mountain-chains, whatever may be their extent.

SAUSSURE, in his Agenda, has already cautioned the geologist against too hasty an inference from the horizontality of the lines of stratification on the surface of a precipitous crag: he has shown that, before pronouncing a mountain to be formed of horizontal strata, it is necessary to view it from the extremities of two lines crossing each other at an angle.

The following considerations will show that SAUSSURE'S observation is not applicable only to the case on which the seams of the strata, when observed from only one side of a mountain, appear horizontal, but that it applies equally well to inclined lines of stratification, which make with the horizon an angle smaller than the true angle of dip of the planes.

Let us suppose a system of parallel planes or strata more or less inclined, into which a section should be made by a vertical plane, whose direction or line of common intersection with the horizontal plane would be parallel to the direction of the strata themselves. In such a case, the seams of the strata on the surface laid bare by the section would appear horizontal. If, on the contrary, the direction of the vertical section was supposed to be perpendicular to the direction of the strata, then the seams of the strata would exhibit a dip identical with the true dip of the planes themselves. But between these two limits an indefinite number of vertical sections may be supposed, in which the angle of dip of the seams will vary according to the direction of the section. The more this direction approaches to a parallelism with the direction of the strata, the lesser will be the angles made by

the seams of the strata with the horizon; the more, on the contrary, the direction of the section approaches to be perpendicular to that of the strata, the greater will be the inclination of the seams.

Now, although the position of the strata in a mountain-chain often remain for a great extent of country invariably the same, the varying direction of the accidental sections will make the lines of the stratification appear changing at every step. Hence arises often the mistaken ideas that seems to prevail, that it is of little use to notice, or mark down upon geological maps, the apparently too variable position of the strata. While such information would be of the greatest interest, whenever questions relating to the upheaving of the strata, and of the chain itself, should happen to be examined.

Of some erroneous opinions to which a neglect of the preceding considerations may have given rise, I shall only notice the following instance.

It is well known that, at some distance to the south-west of Geneva, the River Rhone makes its way through the Jura chain by a narrow and rocky defile. This pass between the two mountains Credo and Varache, is named after the Fort de l'Ecluse, a small but strong fortress, built in this place, to guard this part of the French frontiers. The direction of the strata of Jura limestone, which have been cut through to make way for the river, is nearly north and south, and the direction of the channel in which the river flows nearly east and west; the dip of the strata is to the east; and if the two high rocks which form the sides of the channel had been exactly parallel, the direction of this accidental section would have shown, on both sides, a similar inclination of the seams of the strata corresponding with the dip of the strata themselves to the east. But as the canal is narrower at its eastern extremity than at its western, its two sides are not parallel but converge at the entrance towards the east. Hence

it follows, that in the cliff on the northern bank of the Rhone, the seams present an inclination a little to the south of east, while the seams on the cliff of the southern bank shew a dip a little to the north of east.

From such an appearance some observers have been led to conclude, that this opening in the chain had been produced by a depression of the strata. But the remarks before stated show that such appearances are merely superficial and external, and that a similar inclination in the lines of stratification does not indicate a corresponding inclination of the strata, but is owing simply to the mere accidental direction of the section through the stratified mass. In fact, the strata on both sides of the Rhone, at the Fort de l'Ecluse, although they have been traversed by a wide and deep fissure, entirely occupied by the waters of the river, have not experienced any change in their original direction and dip; so that this narrow transversal valley cannot by any means be called a valley of depression. I am convinced that the same will be found to be the case with many much larger and much more important transversal valleys of the Alps, which have been supposed to be formed by the sinking of the strata on both sides.

Now, it has occurred to me, that such external appearances as are the position of the seams of strata on the surface of cliffs, could enable us, when combined together, to determine the true position of the planes to which they belong, inasmuch as the position of a plane is determined by that of two lines in the same place.

I am indebted to my learned friend M. GAUTIER, Professor of Astronomy in the Academy of Geneva, for the following exact and complete solution of the given problem by means of algebraical formulæ.

The angles α and α', which two straight lines form with their horizontal projection, being given, as well as the angles β and β',

which these projections form with a fixed horizontal axis, there will be determined, 1st, the angle γ comprised between the plane passing through the two straight lines and the horizontal plane by the formula.

$$\tan \gamma = \frac{\sin (\alpha' + \alpha) \cos \phi}{\cos \alpha' \cos \alpha \sin (\beta' - \beta)}$$

ϕ being an auxiliary angle, such that

$$\sin \phi = \frac{\cos \frac{1}{2} (\beta' - \beta) \sqrt{\sin 2 \alpha' \sin 2 \alpha}}{\sin (\alpha' + \alpha)}$$

2d, the angle ω comprised between the line of intersection of the two planes and the fixed axis by the formula

$$\sin (\beta - \omega) = \frac{\text{tang } \alpha}{\text{tang } \gamma}$$

in which every thing is known except ω, when γ has already been determined.

It will be readily perceived that this solution, however satisfactory it may be in a theoretical point of view, will never be advantageously employed in practice, on account of its presupposing that the *data* given by actual observation should be of the nicest accuracy. The measurement of the angle of dip of the lines of stratification, and of the angle which the horizontal projection of these lines make with the magnetic meridian, especially when taken at a distance, can never reach the degree of accuracy required by the nature of the formulæ employed.

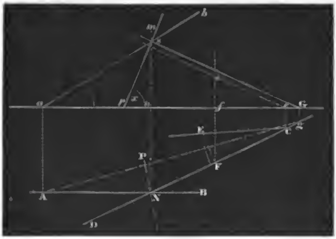
In such a case, practical geometry, or that system of combined projections which is known in French under the name of *Geometrie descriptive*, requiring less perfect *data*, will be in general better adapted to the imperfect means which the geologist possesses for ascertaining the *data* required, and at the same time will give results sufficiently accurate for the present purposes of the science.

M. Dufour of Geneva, well known by his writings as a military engineer, has, at my request, pointed out to me the simple graphical process which I am now to explain, and which may be employed in all cases whatever, where the position of two lines in the same plane is given.

The *data* in this case are the same as in the preceding, viz. the angles which two seams of the strata taken in two different sections of the rock, make with their horizontal projection, and the angle which each of these projections make with the magnetic or with the true meridian.

To determine with the aid of these *data* the true dip and direction of the plane, it is necessary, first, to bring the angles into the same horizontal plane, then through the intersection of two vertical planes passing through the horizontal projections of the seams, to draw a vertical plane. This plane will cut the plane containing the two seams, and the horizontal plane, at an angle, which is the angle required.

Fig. 1.



In Fig. 1. the vertical plane *a b* G is placed parallel to the horizontal projection A B of one of the seams. The point A is

first to be projected vertically as well as the angle G *a b*, which the seam having for its horizontal projection the line A B makes with the horizon.

The known angle ECF, which is seen drawn on the horizontal plane, is to be projected vertically; for which operation it is necessary that it should be made to turn upon DC till it should be returned in its true position, then the point E will be projected horizontally in F and vertically in *e*. The line *e f* will be the vertical projection of EF, and the angle *e c f* will be the vertical projection of the angle ECF.

Two vertical planes are then drawn through the horizontal projections of the seams, and the intersection of these two planes, which is a line perpendicular to the horizontal plane, is vertically projected according to the line *m n*. The point where the line *m n* meets with the line *a b* will be the vertical projection of the point where the two horizontal projections of the seams meet.

The point *s* being common to the two lines, the angle *e c f* is made to slide till the line *b c* meets the point *s*. The angles will be then in the same plane, without their size being altered.

The point G being projected in *g*, the point where the seam, having for its horizontal projection the line DC, cuts the horizontal plane, will be found, and A *g* will be a horizontal line drawn upon the plane passing through the two seams, or, in other words, will be the true direction of the strata.

Nothing remains now but to find the angle of dip, which will be easily obtained in drawing a line perpendicular to the direction or line A *g*, and passing through the line of intersection. Its horizontal projection will be found in drawing from the point N the perpendicular NP, so that a triangle will be formed, of which two sides are known, viz. SN and PN, which will be brought to *n p*, and the angle S *p n* will be the angle made with the horizon by the plane passing through the two seams. PN will be the horizontal projection of the dip.

Fig. 2.

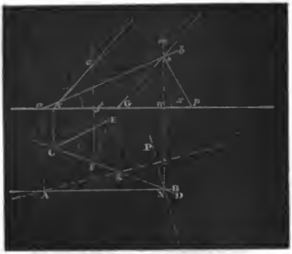
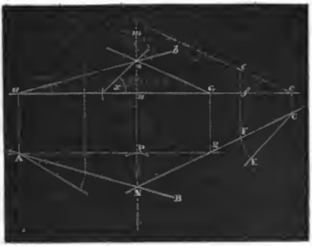


Fig. 3.



The Figures 2. and 3. are intended to show, that whatsoever may be the direction of the horizontal projection of the seams, and whatever may be the position of the vertical plane of the drawing in relation to these projections, the same process will lead to the wished-for result.

Fig. 4.

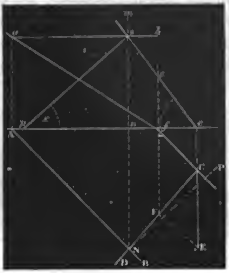
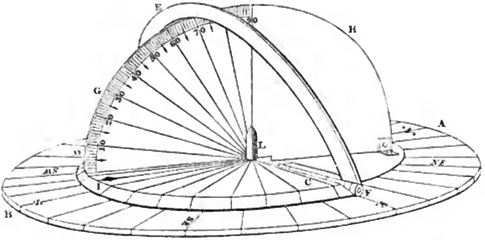


Fig. 4. shows the operation which takes place, in case of the true direction of the strata being one of the given lines of stratification, or seams, in such a case where the angle made by this line with its horizontal projection $= 0$, the process is much simplified, although always carried on according to the same principle. The same letters being used in all the four figures, to denote the analogous points, the explanation above given will apply to them all.

Although this last way of resolving the problem did not require any more precise *data* than those which the less skilled observer can easily obtain in confining himself to the simple use of the compass and the plumb-line; and although this proceeding requires but a little attention, and the use of the most elementary processes of geometry, it is nevertheless to be feared that the practical geologist, to whose care the solutions of such often important problems are trusted, would often think it too long and too complicated a method to be prevailed upon to use it.

Such an idea, and at the same time the wish that numerous and accurate observations of the position of the strata should be

given by all those who have the opportunity of making geological observations in any part of the world, has induced me to contrive a very simple mechanical apparatus, by which all such problems could be easily, and in a very short time, solved. It is this instrument which I call the *Clinometrical Compass*, of which I am now to give the description, at the same time that I have the honour to lay it before the Royal Society.



This instrument is composed of a circular plate of brass A B, divided in thirty-two parts, corresponding to as many points of the compass. A semicircular plate of brass IC, concentric to the above mentioned circle, is made to turn upon it around the common centre. This semicircle is also divided by lines in sixteen divisions, exactly corresponding to those of the under circle. A semicircular portion of a ring of brass E, concentric with both plates, is connected by a hinge F, with the diameter of the semicircular plate, in such a way that the diameter of this plate and that of the half ring are made to coincide in the hinge. In this manner the ring may be moved upwards or downwards, so as to take any inclination whatever upon the horizontal plane, or upon the plane of the lower circular plate, by which the horizontal

plane is represented, and at the same time it may be made to revolve round the centre of the instrument, so as to be placed in any required position relatively to any of the points of the compass.

In such a state of things, the half ring will represent the plane of a stratum, its hinge the direction of the stratum, or its common intersection with the horizontal plane. And all the imaginary lines which may be drawn from the centre of the instrument to the circumference of the ring represent all the possible sections which may be made by vertical planes in the plane of the stratum, and so correspond exactly to the seams or lines of stratification often mentioned above. The divisions on the semicircular plate under the half ring will represent the horizontal projection of those lines which coincide with the given divisions of the compass.

Now, by means of a graduated arc, applied perpendicularly to the plane of the semicircle, and touched in one point by the sharpened interior edge of the half ring, we will be able to measure the angle made by any line in the plane of the half ring with its horizontal projection.

I am indebted to Mr Adie, member of this Society, for a material improvement, that of substituting a simple half ring, such as I have described, for a semicircular plate traversed by fissures, by which I had at first thought of representing the plane of the stratum. To him also is due the contrivance by which the centre of the divided arc is made to coincide with that of the instrument, and the arc itself to stand vertically, which is absolutely necessary in all cases. For these purposes, a small projecting pin occupies the centre of the instrument, and is received into a small hole corresponding to the centre of the protractor, while a small support fixed at a point of its diameter contributes also in keeping it in a vertical position.

An instance will now show the mode of using the instrument,

which, though it may be thought at first rather complicated, will soon be attained by a little practice, and found very simple.

Let the given lines or seams be,

1st, A line dipping 45° S. b. E., or rising 45° N. b. W., which is the same thing.

2d, A line dipping 56° E. b. N., or rising the same number of degrees W. b. S.

We have, first, to observe, that, as has been said before, the hinge or diameter of the brass semicircle correspond to the direction of the stratum, in which direction all the seams appear horizontal. Secondly, that the line marked on the semicircle perpendicularly to the hinge or diameter, is the horizontal projection of the true line of dip or inclination of the stratum, and that this true line of dip is, of all the lines which may be drawn in the plane, that which makes the greatest angle with its horizontal projection. The horizontal projection of this line perpendicular to the hinge I shall name the *Index Line.*

Now, we must begin by supposing, that, of the two lines given, the one that dips with the greater angle may be the true line of dip, and accordingly we will direct the index line to the W. b. S. of the brass circle; then placing the protractor vertically on the index line, we will move upwards the half ring till its sharpened edge comes in contact with the 56th degree of the vertical arc. Looking then to the direction of the other given line, or to the N. b. W. of the compass, and moving the protractor around the centre till it comes to be vertical upon this line, we will look whether the edge of the ring touches its 45th degree, for, in that case, the position of the ring would correspond exactly to that of the required plane. But, in the present instance, it is not necessary even to move the protractor to see that the N. b. W. line is that which corresponds to the dia-

meter, and in consequence to the direction of the stratum; so that in this case the given line, instead of rising 45° above the horizon, ought to be horizontal. The position of the ring does not then correspond to the position of the required plane. This first operation teaches us two things, 1st, That the angle of dip is greater than 56°, so that the half ring may be made to turn upwards upon its hinge. 2d, That the semicircle ought to turn upon its centre, in such a way that the index line shall move gradually towards the west, and afterwards towards the north, in order to bring the N. b. W. line nearer to that of the true dip, to get a line or seam of a greater angle of inclination.

Let us then raise the half ring two degrees more, and turn the semicircle two points of the compass towards the N.W., so that the index line will correspond to the W. b. N. The dip of the line corresponding to the W. b. S. will still be 56°; and if that of the line corresponding to the N. b. W. be 45°, we shall have found the true plane, directed from N. b. E. to S. b. W., and rising 58° towards the W. b. N. Let then the protractor be placed vertically on the line N. b. W., the angle found being 34° instead of 45°, we shall have still to raise the half ring, and move the semicircle in the same direction as before.

In raising the half ring to 60°, and pointing the index line to W.N.W., we shall find a dip of 45° on the N. b. W. line, and of 56° on the W. b. S. one, corresponding with our data. So that we shall have determined the direction of the stratum to be S.S.W. and N.N.E., and its inclination 60° rising to the W. N.W. or dipping to the E.N.E.

If the direction of the plane, or that of a horizontal seam S.S.W. and N.N.E., had been one of the given lines, the operation would have been much simplified; it would have been required only to bring the hinge in that direction, remembering, however, that the other given point of the compass should be comprised in the points embraced in the semicircle; then to

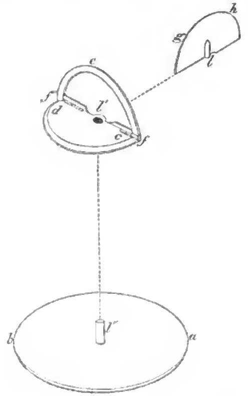
place the protractor vertically on the N. b. W. line, for instance, and to raise the half ring till it should touch the 45th degree; finally, to place the protractor on the index line, indicating the rise to be towards the W.N.W., and to note the angle = 60°.

We hope that this simple little mechanical contrivance will be of use to the practical geologist, who may not have the means or leisure to use the more accurate modes of determination before described. The habit which he will acquire of giving his attention to observations connected with this subject of inquiry, will enable him not only to ascertain in all cases the exact position of the strata whenever they are real planes, but also to discover when two distinct sets of strata are lying in the same plane, and if not, to recognise modes, hitherto little attended to, of unconformable stratification.

Finally, although all the modes of determination above alluded to are only appropriated to the plane strata, the readiness of tact and eye-sight which such considerations will have given to the geologist, will not be without use, when he shall have to study the waving disposition of strata with curved surfaces, whether they are parts of parallel portions of cylinders with their axes horizontal, in which case the seams parallel to these axes or the direction will also appear horizontal, as is observed in that part of the Lammermuir Hills which forms the coast of Berwickshire; or whether they are parts of oblique cylinders with their axes inclined to the horizon, in which case there is no seam which can appear horizontal, because there is a dip even in the direction of the bearing. Of this case the Alps show an amazing variety of instances, among which the more or less dismantled oblique cylindrical structure of the Mount Saléve near Geneva, and of the whole mountain group on the southern bank of the Arve, between Bonneville and Sallenches, are the most instructive and worthy of remark.

Postscript.

As it is necessary that the protractor should stand perfectly vertical, another mode of adjusting it to the centre of the instrument must be devised, instead of that which is alluded to in the paper, and exemplified by the instrument itself. For this purpose two different methods may be adopted. The first is represented in the annexed figure; the brass circle A B or *a b*, should



bear as its central part a solid brass cylinder *l″* of at least two or three lines in length, and one or two in diameter, it would be received in a hole *l′* of the same diameter in the half circle *ed*.

And the protractor *g h* would have at its central part a hollow tube *l*, fitting exactly to the cylinder *l''* of the brass circle *a b*.

The second method, which is still better, and has been suggested to me by Mr ROBISON, is that of adapting the brass cylinder at the centre L or *l* of the protractor. This cylinder should be made to enter into a brass cylindrical tube occupying the centre *l'* of the half circle *c d*, and projecting under the half circle to a length equal to the thickness of the brass circle A B or *a b*. This tube would be received in a hole of equal size corresponding to the central part *l''* of the circle *a b*.

On the Equations of Loci traced upon the Surface of the Sphere, as expressed by Spherical Co-ordinates. By Thomas Stephens Davies, Esq. F. R. S. Lond. & Ed. F. R. A. S.

(Read 1st April 1833).

During the researches which I have made upon this subject since the composition of my former paper, printed in the first Part of this Volume, so many and so varied inquiries arising out of it, and essentially connected with it, have presented themselves, that I have been compelled to make a total change in the plan I had then laid down for the completion of the present section of my communication. I found that many subjects to which I there alluded might, with propriety, be omitted in the present case, as constituting little more, in reference to principles, than illustrations, however interesting they might be when viewed as properties of geometrical figures. On this ground, therefore, I have cancelled a considerable number of properties of the Spherical Conic Sections, and retained only one or two for the purpose of illustrating the method of discussing the properties of those curves. The remarks I intended to make upon the singular points of spherical curves, the geometrical signification of certain symbols, and other inquiries collateral to these, have grown into systems of themselves, or been attached to other dissertations (either wholly or partially completed), to which they seemed to be as closely allied as even to the present subject. From this paper, too, as originally written, I have abstracted considerable portions, amongst which are three distinct classes of spherical research; and I have here confined myself to the essential parts that belong to the subject of spherical co-or-

dinates, under the aspect which they presented to my mind in the first place. Other methods have since exhibited themselves, and the application of analogous methods to other surfaces besides the sphere; but I cannot enter upon them here, though I have developed them to a considerable extent, and applied them to a variety of purposes. The employment of my method of spherical geometry to *physical inquiries* has not escaped my attention; and I look forward with great confidence to its furnishing its aid to many branches of natural philosophy, so as to completely remodel the mathematical methods of conducting those researches.

Owing to severe indisposition, which prevented my giving proper attention to the *proof-sheets* when my former paper was transmitted to me from Edinburgh, several errata have occurred, and I here take the opportunity of indicating the corrections. I have also added a few emendations of passages in that paper, supplied certain material omissions, and corrected one faulty process that occurs there.

I ought also to add, that the present paper was written in its unabridged state more than three months ago; and that in the preparation of it for the Royal Society, no addition or alteration has been made, except what is merely verbal, the omissions alone excepted.

Bath, *Feb.* 24. 1833.

XXXVIII.

EQUATIONS OF TANGENTS, &c.

Hitherto our attention has been directed chiefly to those loci which fulfil certain definite conditions, but which were nevertheless capable of being discussed with very slight appeal to the higher calculus. We shall now proceed, however, to employ the differential and integral calculus for all those purposes *in sphæro*, which are in any way analogous to the uses which they have fulfilled in the geometry of rectilinear and polar co-ordinates, both in two and three dimensions. The value of any system of conducting an inquiry must be determined by its efficiency and its simplicity combined, and these are qualities which can only be determined by direct experiment. In order, then, to accomplish this purpose in the most effectual manner, we shall deduce a series of formulæ for that elementary expression which enters into all these inquiries as a fundamental term—I mean the inclination of the tangent of a curve to the radius-vector in polar curves, and to the axis of co-ordinates in the other case. We shall then be able to find expressions for the various lines that can be drawn in specific modes relative to the tangent at a specific point of the curve, in case both of rectangular and polar equations, and the equations of the tangent and the normal in reference to every kind of co-ordinate axes that we have discussed. From the results thus furnished, we shall be able to select the most advantageous for the particular inquiries we may be engaged in, as well as deduce some general principles to guide us in the choice of the co-ordinates which offer the best prospect of becoming successful in any specific class of loci we may wish to investigate.

We shall commence by means of polar co-ordinates.

1. Let $f(\phi\,\theta)=0$ be the polar equation of a spherical curve; then, if η be the increment which θ (the independent variable) receives, the corresponding value of ϕ becomes, by TAYLOR's Theorem,

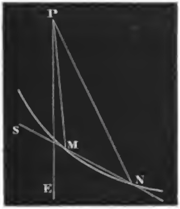
$$\phi'=\phi+\frac{d\phi}{d\theta}\cdot\frac{\eta}{1}+\frac{d^2\phi}{d\theta^2}\cdot\frac{\eta^2}{1.2}+\frac{d^3\phi}{d\theta^3}\cdot\frac{\eta^3}{1.2.3}+\dots\dots\dots\dots(1.)$$

The θ in this is measured on a circle whose polar distance is ϕ; but if we wish to substitute for it the θ of the equator, the latter θ and its differentials must be multiplied by $\sin\phi$ and its powers corresponding with the powers of the several differentials themselves in the denominator. Then (1.) will be changed into

$$\phi' = \phi + \frac{d\phi}{\sin\phi\, d\theta}\cdot\frac{\eta}{1} + \frac{d^2\phi}{\sin^2\phi\, d\theta^2}\cdot\frac{\eta^2}{1.2} + \frac{d^3\phi}{\sin^3\phi\, d\theta^3}\cdot\frac{\eta^3}{1.2.3} + \ldots\ldots\ldots \quad (2.)$$

2. *To find the inclination of a given chord of a spherical curve to the radii-vectores of its extremities.*

Let MN be the chord; $PM = \phi$; $PN = \phi'$; $EPM = \theta$, and $EPN = \theta'$.



Then, by spherics,

$$\tan PMN = \frac{\sin \overline{\theta' - \theta}}{\cot\phi' \sin\phi - \cos\overline{\theta'-\theta}\cos\phi}$$

$$= \frac{\sin\overline{\theta'-\theta}\sin\phi'}{\cos\phi'\sin\phi - \cos\phi\sin\phi'\cos\overline{\theta'-\theta}} \ldots\ldots\ldots\ldots (3.)$$

But $\cos\overline{\theta'-\theta} = 1 - 2\sin^2\frac{1}{2}\overline{\theta'-\theta}$, which inserted in (3.) converts it into

$$\tan PMN = -\frac{\sin\phi'\sin\overline{\theta'-\theta}}{\sin\overline{\phi'-\phi} - 2\sin\phi'\cos\phi\sin^2\frac{1}{2}\overline{\theta'-\theta}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (4.)$$

3. *From this we can find the inclination of the radius-vector to the tangent.*

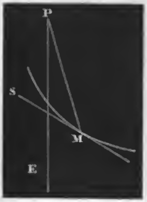
Let $\overline{\theta'-\theta}$ be put $= \eta$. Then, by Taylor's Theorem, (xliii. 1.), (4.) becomes

$$\tan \mathrm{PMN} = -\frac{\sin \eta \sin\left\{\phi + \frac{d\phi}{d\theta}\cdot\frac{\eta}{1} + \frac{d^2\phi}{d\theta^2}\cdot\frac{\eta^2}{1\,.\,2} + \dots\right\}}{\sin\left\{\frac{d\phi}{d\theta}\cdot\frac{\eta}{1} + \frac{d^2\phi}{d\theta^2}\cdot\frac{\eta^2}{1\,.\,2} + \dots\right\} - 2\cos\phi\,\sin^2\tfrac{1}{2}\,\eta\,\sin\left\{\phi + \frac{d\phi}{d\theta}\cdot\frac{\eta}{1} + \dots\right\}} \dots(5.)$$

Now, as η is diminished, tan PMN approximates towards $-\frac{\sin d\phi \sin d\,\theta}{\sin d\phi}$; and at the limit we have actually

$$\tan \mathrm{PMN} = -\frac{\sin \phi d\theta}{d\phi} \dots\dots\dots\dots \dots\dots\dots (6.)$$

putting $d\phi$ and $d\theta$ for $\sin\phi$ and $\sin d\theta$. Hence, in reference to the case of ultimate approximation of PM and PN (that is, when they are coalescing, as at M), we have



$$\tan \mathrm{PMS} = \frac{\sin \phi d\theta}{d\phi} \dots\dots\dots\dots\dots\dots\dots\dots (7.)$$

which is the same result as we obtained as that which has been long well known by those geometers who have treated of the length of the loxodrome *, and which has been used in my paper (XVI. 2.)

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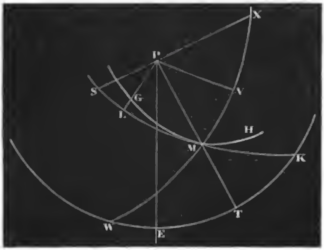
* So far as I have been able to discover, this formula is due to EULER, who employed it in a paper on "Spherical Trigonometry, by the method of *maxima* and *minima*," in the Berlin Memoirs for 1753, p. 226. It is very remarkable how near EULER approached, in the passage here referred to, to the method of spherical co-ordinates, such as we have here developed; and yet he does not seem to have entertained the slightest notion that such a method of investigation was capable of general application. Had it once occurred to his mind, there is no doubt that Spherical Geometry would have been in a much more advanced state than it now is. Its principles have been fully developed, and its practice rendered familiar as a branch of elementary study.

From this formula, expressions for all the great circles, segments, and spherical angles that enter into the figure, will be presently derived.

XXXIX.

The several other parts of the figure may be obtained by the simple application of NAPIER's rules to the right-angled triangles that constitute the system of mutually dependent quantities. The manner of obtaining them is so obvious, that I shall omit all indication of it here, and merely tabulate the results for the purpose of reference in future inquiries.



1st, *Equatoreal or Geographical Co-ordinates.*

Let PE, EK be the axes of co-ordinates, that is, the meridian and equator of reference. Then, MK being the tangent at M to a spherical curve, and MW the normal, if we put $ET = \theta_{,}$, and $TM = \phi_{,}$, we shall obtain TABLE I.

TABLE I.

PARTS OF THE FIGURE REFERRED TO GEOGRAPHICAL CO-ORDINATES.

PARTS.	SINE.	COSINE.	TANGENT.
TMK	$\frac{+\cos\varphi_{,}\, d\theta_{,}}{\sqrt{d\varphi^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{-d\varphi_{,}}{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{\cos\varphi_{,} d\theta_{,}}{d\varphi_{,}}$
TKM	$\sqrt{\frac{d\varphi_{,}^2+\cos^2\varphi_{,} \sin^2\varphi_{,} d\theta_{,}^2}{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{+\cos^2\varphi_{,} d\theta_{,}^2}{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$+\frac{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} \sin^2\varphi_{,} d\theta_{,}^2}}{\cos^2\varphi_{,} d\theta_{,}}$
WMT	$\frac{-d\varphi_{,}}{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{+\cos\varphi_{,} d\theta_{,}}{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{d\varphi'}{\cos\varphi_{,} d\theta_{,}}$
MWT	$\sqrt{\frac{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{\cos\varphi_{,} d\varphi_{,}}{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{\sqrt{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}{\cos\varphi_{,} d\varphi_{,}}$
TK	$\frac{\cos\varphi_{,} \sin\varphi_{,} d\theta_{,}}{\sqrt{d\varphi_{,}^2+\sin^2\varphi_{,} \cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{d\varphi_{,}^2}{\sqrt{d\varphi_{,}^2+\sin^2\varphi_{,} \cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{\cos\varphi_{,} \sin\varphi_{,} d\theta_{,}}{d\varphi_{,}}$
MK	$\sqrt{\frac{(d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2)\sin^2\varphi_{,}}{d\varphi_{,}^2+\sin^2\varphi_{,} \cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{-\cos\varphi_{,} d\varphi_{,}}{\sqrt{d\varphi_{,}^2+\sin^2\varphi_{,} \cos^2\varphi' d\theta_{,}^2}}$	$-\frac{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}{\cot\varphi_{,} d\varphi_{,}}$
TW	$\frac{-\sin\varphi_{,} d\varphi_{,}}{\sqrt{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{\cos\varphi_{,} d\theta_{,}}{\sqrt{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$-\frac{\tan\varphi_{,} d\varphi_{,}}{d\theta_{,}}$
MW	$\sqrt{\frac{(d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2)\sin^2\varphi_{,}}{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{\cos\varphi_{,} d\theta_{,}}{\sqrt{\sin^2\varphi_{,} d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}$	$\frac{\sqrt{d\varphi_{,}^2+\cos^2\varphi_{,} d\theta_{,}^2}}{\cos\varphi_{,} \cot\varphi_{,} d\theta_{,}}$

To adapt these quantities to the case of polar or astronomical co-ordinates, referred to pole P, and prime meridian PE, we have only to change $\phi_{,}$ into $\frac{\pi}{2}-\phi$, and cancel the subscribed accent from $\theta_{,}$. This interchanges $\sin\phi$ and $\cos\phi$: and also $d\phi$ and $-d\phi$, or all the signs in the above Table become $+$.

2d, *Referred to Astronomical or Polar Co-ordinates.*

Let P and PE be the pole and meridian of reference; then, if the perpendiculars PL, PV, and SPX be drawn to the tangent, normal and radius-vector (that is, to MS, MX, and MP), we shall have PS, PX the subtangent and subnormal, and PL, PV the perpendicular from the pole upon the tangent and normal. Put $EPM = \theta$ and $PM = \phi$. Then the values of the several functions of the parts of the figure will be as in the following Table.

TABLE II.

PARTS RELATIVE TO THE TANGENT.

PARTS.	SINE.	COSINE.	TANGENT.
LPM	$\frac{d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\cos^2\varphi\, d\theta^2}}$	$\frac{\sin\varphi\cos\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\cos^2\varphi\, d\theta^2}}$	$\frac{d\varphi}{\sin\varphi\cos\varphi\, d\theta}$
PSM	$\sqrt{\frac{d\varphi^2 + \sin^4\varphi\, d\theta^2}{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\cos\varphi\sin\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sqrt{d\varphi^2 + \sin^4\varphi\, d\theta^2}}{\cos\varphi\sin\varphi\, d\theta}$
SMP	$\frac{\sin\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sin\varphi\, d\theta}{d\varphi}$
ML	$\frac{\sin\varphi\, d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\cos^2\varphi\, d\theta^2}}$	$\sqrt{\frac{(d\varphi^2 + \sin^2\varphi\, d\theta^2)\cos^2\varphi}{d\varphi^2 + \sin^2\varphi\cos^2\varphi\, d\theta^2}}$	$\frac{\tan\varphi\, d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$
MS	$\sqrt{\frac{(d\varphi^2 + \sin^2\varphi\, d\theta^2)\sin^2\varphi}{d\varphi^2 + \sin^4\varphi\, d\theta^2}}$	$\frac{\cos\varphi\, d\varphi}{\sqrt{d\varphi^2 + \sin^4\varphi\, d\theta^2}}$	$\frac{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}{\cot\varphi\, d\varphi}$
PS	$\frac{\sin^2\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^4\varphi\, d\theta^2}}$	$\frac{d\varphi}{\sqrt{d\varphi^2 + \sin^4\varphi\, d\theta^2}}$	$\frac{\sin^2\varphi\, d\theta}{d\varphi}$
PL	$\frac{\sin^2\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\sqrt{\frac{d\varphi^2 + \cos^2\varphi\sin^2\varphi\, d\theta^2}{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sin^2\varphi\, d\theta}{\sqrt{d\varphi^2 + \cos^2\varphi\sin^2\varphi\, d\theta^2}}$

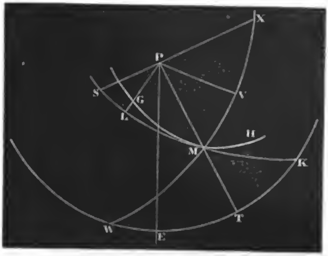


TABLE II.—*concluded.*

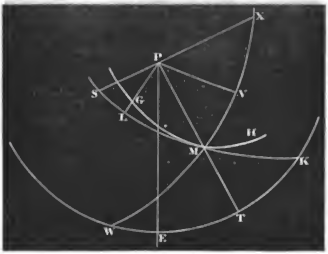
PARTS RELATIVE TO THE NORMAL.

PARTS.	SINE.	COSINE.	TANGENT.
MPV	$\frac{\sin\varphi\, d\theta}{\sqrt{\cos^2\varphi\, d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\cos\varphi\, d\varphi}{\sqrt{\cos^2\varphi\, d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\tan\varphi\, d\theta}{d\varphi}$
MXP	$\sqrt{\frac{(d\varphi^2 + d\theta^2)\sin^2\varphi}{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\cos\varphi\, d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sqrt{d\theta^2 + d\varphi^2}}{\cot\varphi\, d\varphi}$
PMX	$\frac{d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sin\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{d\varphi}{\sin\varphi\, d\theta}$
MV	$\frac{\sin^2\varphi\, d\theta}{\sqrt{\sin^2\varphi\, d\theta + \cos^2\varphi\, d\varphi^2}}$	$\sqrt{\frac{(d\varphi^2 + \sin^2\varphi\, d\theta^2)\cos^2\varphi}{\sin^2\varphi\, d\theta^2 + \cos^2\varphi\, d\varphi^2}}$	$\frac{\tan\varphi\sin\varphi\, d\theta}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$
MX	$\sqrt{\frac{d\varphi^2 + \sin^2\varphi\, d\theta^2}{d\varphi^2 + d\theta^2}}$	$\frac{\cos\varphi\, d\theta}{\sqrt{d\varphi^2 + d\theta^2}}$	$\frac{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}{\cos\varphi\, d\theta}$
PV	$\frac{\sin\varphi\, d\varphi}{\sqrt{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\sqrt{\frac{\cos^2\varphi\, d\varphi + \sin^2\varphi\, d\theta^2}{d\varphi^2 + \sin^2\varphi\, d\theta^2}}$	$\frac{\sin\varphi\, d\varphi}{\sqrt{\cos^2\varphi\, d\varphi^2 + \sin^2\varphi\, d\theta^2}}$
PX	$\frac{d\varphi}{\sqrt{d\varphi^2 + d\theta^2}}$	$\frac{d\theta}{\sqrt{d\varphi^2 + d\theta^2}}$	$\frac{d\varphi}{d\theta}$

XL.

The equations of the tangent and normal to a spherical curve referred to polar co-ordinates.

1. *The tangent* SMK *at the point* M, *or* $\phi' \theta'$.



Let the polar *astronomical* co-ordinates be PE and EPM. Correcting the sign of $\sin\theta$ in XI. 9. as directed at p. 35, putting for $\alpha\beta$ their value in the present case $\phi'\theta'$; and for $\sin\epsilon$, $\cos\epsilon$, their values in terms of $\phi'\theta'$ furnished by Table II. of section XXXIX.: then we shall have for the equation of the tangent

$$\cot\phi = -\operatorname{cosec}\alpha\operatorname{cosec}\epsilon\left\{\begin{array}{l} -\cos\theta\,(\sin\beta\cos\epsilon + \sin\epsilon\cos\beta\cos\alpha) \\ +\sin\theta\,(\cos\beta\cos\epsilon - \sin\epsilon\sin\beta\cos\alpha)\end{array}\right\}$$

$$= - \quad \operatorname{cosec}^2\phi'\left\{\begin{array}{l} -\cos\theta\,\left(\sin\theta'\dfrac{d\phi'}{d\theta'} + \cos\theta'\sin\phi'\cos\phi'\right) \\ +\sin\theta\,\left(\cos\theta'\dfrac{d\phi'}{d\theta'} - \sin\theta'\sin\phi'\cos\phi'\right)\end{array}\right\} \text{.........(1.)}$$

and this is readily changed into the form,

$$-\sin\overline{\theta'-\theta}\,\frac{d\phi'}{d\theta'} + \cos\overline{\theta'-\theta}\sin\phi'\cos\phi' + \sin^2\phi'\cot\phi = 0 \text{ ....(2.)}$$

We may change this at once into *geographical* co-ordinates, by writing

$\frac{\pi}{n} - \phi$, instead of ϕ and accenting θ' in the same manner; thus we have at once

$$\sin \overline{\theta_{,} - \theta} \frac{d\phi_{,}}{d\theta_{,}} + \cos \overline{\theta_{,} - \theta} \sin \phi_{,} \cos \phi_{,} + \cos^2 \phi_{,} \tan \phi = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots (3.)$$

2. *The normal* XMV, *at the point* M, *or* $\phi'\theta'$.

Let the *astronomical* and *geographical* co-ordinates be denoted as before. Then, in the former case, instead of the tangent adapted to ϵ, we must employ that for $\frac{\pi}{2} + \epsilon$; that is, we must write

$$\sin\left(\frac{\pi}{2} + \epsilon\right) = \cos \epsilon = \frac{d\phi'}{\sqrt{d\phi'^2 + \sin^2 \phi' d\theta'^2}} \text{ and}$$

$$\cos\left(\frac{\pi}{2} + \epsilon\right) = -\sin \epsilon = \frac{-\sin \phi' d\theta'}{\sqrt{d\phi'^2 + \sin^2 \phi' d\theta'^2}}$$

These, inserted in XI. 9. (corrected as above), give the following equation.

$$\cot \phi = -\operatorname{cosec} \phi' \left\{ \begin{array}{l} -(\cos\theta \sin\theta' - \sin\theta \cos\theta') \frac{-\sin \phi' d\theta'}{d\phi'} \\ -(\cos\theta \cos\theta' + \sin\theta \sin\theta') \end{array} \right\} \ldots\ldots\ldots\ldots (4.)$$

or which, by reduction, becomes

$$\sin \overline{\theta' - \theta} \sin \phi' \frac{d\theta'}{d\phi'} - \cos \overline{\theta' - \theta} \cos \phi' + \cot \phi \sin \phi' = 0 \ldots\ldots\ldots\ldots\ldots\ldots (5.)$$

Making the requisite change to adapt it to *geographical* co-ordinates, we have

$$-\sin \overline{\theta_{,} - \theta} \cos \phi_{,} \frac{d\theta_{,}}{d\phi_{,}} - \cos \overline{\theta_{,} - \theta} \cos \phi_{,} + \tan \phi \cos \phi_{,} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (6.)$$

XLI.

To find the equation of spherical curves in terms of its radius-vector and the perpendicular from the pole upon the tangent.

Let the perpendicular PL $= \psi$; then, by the table,

$$\sin \psi = \frac{\sin^2 \phi d\theta}{\sqrt{d\phi^2 + \sin^2 \phi d\theta^2}}$$

$$= \frac{\sin^2 \phi}{\sqrt{\sin^2 \phi + \left(\frac{d\phi}{d\theta}\right)^2}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (1.)$$

From this also we get the value of the differential coefficient, viz.

$$\frac{d\phi}{d\theta}=\frac{\sin\psi}{\sin\phi\sqrt{\sin^2\phi-\sin^2\psi}} \quad\text{.......... ....}\ (2.,$$

or

$$\frac{d\sigma}{d\phi}=\frac{\sin\phi}{\sqrt{\sin^2\phi-\sin^2\psi}}\quad\text{............... ....}(3.)$$

where $d\sigma$ is the corresponding element of the curve. By means of (3.) we can eliminate the differentials, and thus obtain the equation of the curve, as *in plano*, in finite relations between ϕ and ψ.

We may illustrate this by one or two examples.

1. *To find the perpendicular from the pole upon the tangent of a spherical conic section, referred to the focus and major axis.*

The equation of a conic section referred to the said co-ordinates, is

$$\tan\phi=\mp\frac{\cos 2a-\cos 2\epsilon}{\sin 2a\mp\sin 2\epsilon\cos\theta}\quad\text{...............................}(4.)$$

Differentiating, we get

$$\frac{d\phi}{\cos^2\phi}=\frac{(\cos 2a-\cos 2\epsilon)\sin 2\epsilon\sin\theta\,d\theta}{(\sin 2a\mp\sin 2\epsilon\cos\theta)^2}, \text{ or by (4)}$$

$$\frac{d\phi}{d\theta}=\frac{\sin^2\phi\sin 2\epsilon\sin\theta}{\cos 2a-\cos 2\epsilon}\quad\text{..................................}(5.)$$

Again, from (4) we find

$$\cos\theta=\frac{(\cos 2a\cos 2\epsilon)\pm\sin 2a\tan\phi}{\sin 2\epsilon\tan\phi}\quad\text{..................}(6.)$$

From this

$$\sin^2\theta=1-\cos^2\theta=\frac{\sin^2 2\epsilon\tan^2\phi-\sin^2 2a\tan^2\phi\mp 2(\cos 2a-\cos 2\epsilon)\sin 2a\tan\phi-(\cos 2a-\cos 2\epsilon)^2}{\sin^2 2\epsilon\tan^2\phi}$$

$$=\frac{(\cos^2 2a-\cos^2 2\epsilon)\sin^2\phi\mp 2(\cos 2a-\cos 2\epsilon)\sin 2a\sin\phi\cos\phi-(\cos 2a-\cos 2\epsilon)^2\cos^2\phi}{\sin^2 2\epsilon\sin^2\phi}$$

$$=\frac{(\cos 2a-\cos 2\epsilon)\{(\cos 2a+\cos 2\epsilon)\sin^2\phi\mp 2\sin 2a\sin\phi\cos\phi-(\cos 2a-\cos 2\epsilon)\cos^2\phi\}}{\sin^2 2\epsilon\sin^2\phi}$$

$$=\frac{\cos 2a-\cos 2\epsilon}{\sin^2 2\epsilon}\cdot\frac{\cos 2a(\sin^2\phi-\cos^2\phi)+\cos 2\epsilon(\sin^2\phi+\cos^2\phi)\mp 2\sin 2a\sin\phi\cos\phi}{\sin^2\phi}$$

$$=\frac{\cos^2 a-\cos^2\epsilon}{\sin^2 2\epsilon}\cdot\frac{-\cos 2a\cos 2\phi\mp\sin 2a\sin 2\phi+\cos 2\epsilon}{\sin^2\phi}$$

$$=\frac{\cos 2a-\cos 2\epsilon}{\sin^2 2\epsilon}\cdot\frac{\cos 2\epsilon-\cos 2(a\mp\phi)}{\sin^2\phi}\quad\text{...... ....}\ (7.)$$

This value of $\sin\theta$ inserted in (5) gives

$$\frac{d\phi}{d\theta} = \mp \frac{\sin^2\phi \sin 2\epsilon}{\cos 2a - \cos 2\epsilon} \cdot \frac{\{(\cos 2a - \cos 2\epsilon)(\cos 2\epsilon - \cos 2(a \mp \phi))\}^{\frac{1}{2}}}{\sin 2\epsilon \sin\phi}$$

$$= \mp \sin\phi \left\{\frac{\cos 2\epsilon - \cos 2(a \mp \phi)}{\cos 2a - \cos 2\epsilon}\right\}^{\frac{1}{2}} \quad \text{.....................} \quad (8.)$$

But from (1) we have this (8) converted into

$$\sin^2\psi = \frac{\sin^4\phi}{\sin^2\phi + \sin^2\phi \dfrac{\cos 2\epsilon - \cos 2(a \mp \phi)}{\cos 2a - \cos 2\epsilon}}$$

$$= \frac{(\cos 2a - \cos 2\epsilon)\sin^2\phi}{\cos 2a - \cos(2a \mp \phi)}, \text{ or}$$

$$\sin\psi = \pm (\cos 2a - \cos 2\epsilon)^{\frac{1}{2}} \left(\frac{\sin\phi}{\sin(2a \mp \phi)}\right)^{\frac{1}{2}} \quad \text{.........} \quad (9)$$

If we resolve this with respect to ϕ instead of ψ, we shall get

$$\cot\phi = \frac{\cos 2a \cos^2\psi - \cos 2\epsilon}{\sin^2\psi \sin 2a} \quad \text{......................} \quad (10.)$$

whence $\sin\phi$ and $\cos\phi$ may also be obtained.

2. *The perpendicular from any point upon the tangent to a circle.*

As this question refers to the *magnitude* of the perpendicular, but not its *position*, the solution loses nothing of its generality by taking the great circle through the centre of the circle for origin of θ, or, in other words, by taking $\kappa = 0$.* Then we have

$$\cos\rho = \cos\phi\cos\lambda + \sin\phi\sin\lambda\cos\theta \quad \text{.....................} \quad (11)$$

Differentiating, we have

$$\frac{d\phi}{d\theta} = \frac{\sin\lambda\sin\phi\sin\theta}{\cos\phi\cos\theta\sin\lambda - \sin\phi\cos\lambda} \quad \text{........................} \quad (12.)$$

---

* Indeed, had we taken $(\theta - \kappa)$ for the angle made by the radius-vector of the current point in the circle and the first meridian, we should still eliminate the functions of $\overline{\theta - \kappa}$ at step (14), as we have actually done with θ, and the result is therefore exactly the same.

But from (1) we get

$$\cos\theta = \frac{\cos\rho - \cos\phi\cos\lambda}{\sin\phi\sin\lambda}$$

$$\sin\theta = \pm\frac{\{\sin^2\lambda - \cos^2\rho + 2\cos\rho\cos\lambda\cos\phi - \cos^2\phi\}^{\frac{1}{2}}}{\sin\phi\sin\lambda} \quad \text{......(13.)}$$

Or from (12), (13),

$$\frac{d\phi}{d\theta} = \pm\frac{\sin\phi\sqrt{\sin^2\lambda - \cos^2\rho + 2\cos\rho\cos\lambda\cos\phi - \cos^2\phi}}{\cos\phi\cos\rho - \cos\lambda} \quad \text{...... (14.)}$$

And putting this in the value of ψ, we find the value which we sought, viz.

$$\sin\psi = \pm\frac{\cos\phi\cos\rho - \cos\lambda}{\sin\rho} \quad \text{.................. (15.)}$$

If the pole be in the circumference of the circle, then $\lambda = \rho$ and there results

$$\sin\psi = \mp 2\cot\rho\sin^2\tfrac{1}{2}\phi \quad \text{........................ (16.)}$$

XLII.

To find the intersection of the perpendicular from the pole of astronomical co-ordinates upon the tangent to a spherical curve at the point $\phi'\theta'$.

We might employ the general expression found in (X. 10), or rather in the correction to that passage at the end of the present paper: but as that method would, in reference to our present object, be unnecessarily operose, we shall adopt a briefer one. It depends upon this,—that when the radius-vector is perpendicular to a curve (or, in other words, at an apse), cot ϕ will be a *maximum* or a *minimum*, and hence that the equation

$$\cot\phi = -\tan\lambda\cos\overline{\theta - \kappa}$$

differentiated on this supposition, gives

$$\sin\overline{\theta - \kappa} = 0$$

that is, $\theta = \kappa$, or generally $\theta = n\pi + \kappa$ .... (1.)
(where n is a whole number.)

which gives the value of θ corresponding to the perpendicular from the pole upon the tangent.

Recurring then to (XL. 1), we have

$$\left.\begin{aligned}
\tan\kappa &= -\frac{\cos\theta'\dfrac{d\phi'}{d\theta'} - \sin\theta'\sin\phi'\cos\phi'}{\sin\theta'\dfrac{d\phi'}{d\theta'} + \cos\theta'\sin\phi'\cos\phi'} \\
\sin\kappa &= \mp\frac{\cos\theta'\dfrac{d\phi'}{d\theta'} - \sin\theta'\sin\phi'\cos\phi'}{\sqrt{\cos^2\phi'\sin^2\phi' + \left(\dfrac{d\phi'}{d\theta'}\right)^2}} \\
\cos\kappa &= \pm\frac{\sin\theta'\dfrac{d\phi'}{d\theta'} + \cos\theta'\sin\phi'\cos\phi'}{\sqrt{\cos^2\phi'\sin^2\phi' + \left(\dfrac{d\phi'}{d\theta'}\right)^2}}
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

$$\left.\begin{aligned}
\tan\lambda &= \pm\frac{\sqrt{\cos^2\phi'\sin^2\phi' + \left(\dfrac{d\phi'}{d\theta'}\right)^2}}{\sin^2\phi'} \\
\sin\lambda &= \pm\sqrt{\frac{\cos^2\phi'\sin^2\phi' + \dfrac{d\phi'^2}{d\theta'^2}}{\sin^2\phi' + \dfrac{d\phi'^2}{d\theta'^2}}} \\
\cos\lambda &= +\frac{\sin^2\phi'}{\sqrt{\sin^2\phi' + \dfrac{d\phi'^2}{d\theta'^2}}}
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (3.)$$

The last equation of set (3) agrees with (XLI. 1), as indeed it ought to do, being, in fact, an expression for the same line in functions of the same quantities.

When from equations (2), (3), and the equation of the particular curve under consideration, $f(\phi\,\theta) = 0$, we can eliminate ϕ' and θ', we shall obtain an equation in terms of κ, λ, and the constants which enter into $f(\theta\,\phi) = 0$; and this will be the equation of the curve traced out by the foot of the perpendicular on the tangent. We shall hereafter give an example or two.

For the present we shall confine our attention to the general formulæ for the parts of the figure we have been considering.

XLIII.

On Contact and Osculation.

1. *In respect to the equation between ϕ and θ.*

Let the spherical polar curves be denoted by

$$f(\phi\,\theta) = 0 \quad \dots\dots\dots\dots \quad (1.)$$

$$\mathrm{F}(\phi'\,\theta') = 0 \quad \dots\dots\dots\dots \quad (2.)$$

Then the values of the radii-vectores when θ, θ' have received increments κ and κ', will be respectively

$$\phi + \frac{d\phi}{\sin\phi\, d\theta} \cdot \frac{\kappa}{1} + \frac{d^2\phi}{\sin^2\phi\, d\theta^2} \cdot \frac{\kappa^2}{1.2} + \dots\dots\dots \quad (3.)$$

$$\phi' + \frac{d\phi'}{\sin\phi'\, d\theta'} \cdot \frac{\kappa'}{1} + \frac{d^2\phi'}{\sin^2\phi'\, d\theta'^2} \cdot \frac{\kappa'^2}{1.2} + \dots\dots\dots \quad (4)$$

If now for any value of $\theta = \theta'$ we have also $\phi = \phi'$, then $\sin\phi = \sin\phi'$: and if we take $\kappa = \kappa'$, our equations (3, 4) will become

$$\phi + \frac{d\phi}{\sin\phi\, d\theta} \frac{\kappa}{1} + \frac{d^2\phi}{\sin^2\phi\, d\theta^2} \cdot \frac{\kappa^2}{1.2} + \cdot \quad \dots\dots\dots \quad (5.)$$

$$\phi + \frac{d\phi'}{\sin\phi\, d\theta'} \cdot \frac{\kappa}{1} + \frac{d^2\phi'}{\sin^2\phi\, d\theta'^2} \cdot \frac{\kappa^2}{1.2} + \dots\dots\dots \quad (6.)$$

and it will follow (by reasoning exactly similar to that which is used in the case of differential coefficients of a function referred to rectilinear co-ordinates) that the two spirals will have a contact of the order (n) denoted by the number of differential coefficients (n) which are equal in (5) and (6); that no spiral which has less than (n) differential coefficients equal to these can pass between (5) and (6); that when the order of contact is odd, it is contact only, and when even, that it is both contact and intersection; and indeed all the general properties which depend upon the coefficients themselves. I have not, therefore, considered it necessary to put down these ar-

guments at length; but proceed at once to apply them to the peculiar objects before us, that is, for obtaining the formulæ adapted to spherical co-ordinates. These formulæ, it is at once obvious, have an intimate relation with those *in plano*, and often embrace them as particular cases: nevertheless the spherical can never be inferred from the plane, though the plane can most commonly *(still not always so)* be inferred from the spherical expressions.

2. But we may, as is always done in the investigation of spirals *in plano*, employ *the equation between the radius-vector and perpendicular upon the tangent.*

Considering ψ and ψ' as functions of ϕ and ϕ' respectively, we shall arrive by similar considerations to those before used, at the conditions

$$\phi = \phi',\ \psi = \psi',\ \frac{d\psi}{d\phi} = \frac{d\psi'}{d\phi'},\ \ldots\ldots,\ \frac{d^n\psi}{d\phi^n} = \frac{d^n\psi'}{d\phi'^n}\ \ldots\ldots\ldots\ldots \quad (7.)$$

as the test of contact of the ($n+1$th) order, and all the consequences usually drawn will follow from these, as well as from the previous ones.

XLIV.

Radius of Spherical Curvature, Involutes and Evolutes.

By the radius of spherical curvature, I mean the spherical radius of the circle which has a contact of the second order with the given curve, at the current point $\phi\ \theta$, or $\phi\ \psi$, according as we use one or other of these pairs of co-ordinates.

1st, We shall first take the equation between $\psi\ \phi$.

Here $$\mathrm{F}(\phi'\psi') = 0 \ \ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

and the circle of contact (XLV. 15.) is $\sin\psi = \pm\dfrac{\cos\phi\cos\rho - \cos\lambda}{\sin\phi}$ ... (2.)

Now the equation (2) involving the two arbitrary constants ρ and λ, these may be so determined as to fulfil the conditions

$$\sin\psi = \sin\psi' \text{ and } \frac{d\psi}{d\phi} = \frac{d\psi'}{d\phi'}\,.$$

Reducing (2), we have

$$\sin\rho\sin\psi \mp \cos\rho\cos\phi = \mp\cos\lambda\,; \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3.)$$

from which, by differentiating, we get

$$\frac{d\psi}{d\phi} = \mp\frac{\sin\phi\cos\rho}{\cos\psi\sin\rho}\,, \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (4.)$$

Whence

$$\left.\begin{aligned} \tan\rho &= \mp\frac{\sin\phi\, d\phi}{\cos\psi\, d\psi} \\ \sin\rho &= \frac{\pm\sin\phi\, d\phi}{\sqrt{\cos^2\psi\, d\phi^2 + \sin^2\phi\, d\phi^2}} \\ \cos\rho &= \frac{-\cos\psi\, d\psi}{\sqrt{\cos^2\psi\, d\psi^2 + \sin^2\phi\, d\phi^2}} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots \quad (5)$$

Insert these values of $\sin\phi$, $\cos\phi$ in (3), and reduce; then,

$$\left.\begin{aligned} \cos\lambda &= \pm\frac{\sin\psi\sin\phi\, d\phi + \cos\phi\cos\psi\, d\psi}{\sqrt{\cos^2\psi\, d\psi^2 + \sin^2\phi\, d\phi^2}} \\ \sin\lambda &= \sqrt{\frac{\sin^2\phi\cos^2\psi\,(d\psi^2 + d\phi^2) - 2\sin\phi\cos\phi\sin\psi\cos\psi\, d\phi\, d\psi}{\cos^2\psi\, d\psi^2 + \sin^2\phi\, d\phi^2}} \\ \cot\lambda &= \pm\frac{\sin\psi\sin\phi\, d\phi + \cos\phi\cos\psi\, d\psi}{\sqrt{\sin^2\phi\cos^2\psi\,(d\psi^2 + d\phi^2) - 2\sin\phi\cos\phi\sin\psi\cos\psi\, d\phi\, d\psi}} \end{aligned}\right\} \ldots\ldots \quad (6.)$$

whence from (5) we know the radius of curvature, and from (6) the polar distance of the centre.

By restoring the value of ψ in terms of $\phi\,\theta$, we should, of course, obtain the several values in terms of the co-ordinates of the point of contact. This purpose might, however, be easily effected by direct investigation, and we should avail ourselves of that circumstance to form them into mutual checks of the accuracy of our operations and results. Too much attention cannot be given to these verifications, where, as in the present case, the formulæ are destined to become fundamental in respect to a long series of important subsequent inquiries.

Let the circle of curvature be denoted by

$$\cos\rho = \cos\lambda\cos\phi + \sin\lambda\sin\phi\cos\overline{\theta - \kappa} \quad \ldots\ldots\ldots\ldots\ldots \quad (7.)$$

and the curve by

$$F(\phi'\theta') = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (8.)$$

Differentiating (7), we obtain

$$\cot \lambda = \frac{\cos \overline{\theta - \kappa} \cos \phi \, d\phi - \sin \overline{\theta - \kappa} \sin^2 \phi \, d\theta}{\sin \phi \, d\phi} \quad \ldots\ldots\ldots\ldots \quad (9.)$$

$$\text{or } \cot \lambda = \cos \overline{\theta - \kappa} \cot \phi - \sin \overline{\theta - \kappa} \cdot \frac{d\theta}{d\phi} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (10.)$$

Differentiating again, we have

$$\sin \overline{\theta - \kappa} \left\{ \cot \phi \, d\theta + \frac{d\phi \, d^2\theta - d^2\phi \, d\theta}{d\phi^2} \right\} + \cos \overline{\theta - \kappa} \left\{ \frac{d\phi}{\sin^2 \phi} + \frac{d\theta^2}{d\phi} \right\} = 0 \ldots \quad (11.)$$

Or writing it

$$B \sin \overline{\theta - \kappa} + A \cos \overline{\theta - \kappa} = 0 \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (12.)$$

we can successively determine κ, λ, and ϱ in terms of ϕ θ and the differential coefficients.

From (12) we have

$$\left. \begin{aligned} \tan \overline{\theta - \kappa} &= -\frac{A}{B} \\ \sin \overline{\theta - \kappa} &= \frac{\mp A}{\sqrt{A^2 + B^2}} \\ \cos \overline{\theta - \kappa} &= \frac{\pm B}{\sqrt{A^2 + B^2}} \end{aligned} \right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (13.)$$

These inserted in (9) give

$$\left. \begin{aligned} \cot \lambda &= \mp \frac{B \cos \phi \, d\phi + A \sin \phi \, d\theta}{\sin \phi \, d\phi \sqrt{A^2 + B^2}} \\ \sin \lambda &= + \frac{\sin \phi \, d\phi \sqrt{A^2 + B^2}}{\sqrt{A^2 \sin^2 \phi \, (d\phi^2 + d\theta^2) + 2AB \sin \phi \cos \phi \, d\phi \, d\theta + B^2 d\phi^2}} \\ \cos \lambda &= \mp \frac{B \cos \phi \, d\phi + A \sin \phi \, d\theta}{\sqrt{A^2 \sin^2 \phi \, (d\phi^2 + d\theta^2) + 2AB \sin \phi \cos \phi \, d\phi \, d\theta + B^2 d\phi^2}} \end{aligned} \right\} \ldots\ldots \quad (14.)$$

and finally

$$\cos \varrho = \frac{\mp (B \cos \phi \, d\phi + \sin \phi \, d\theta) \cos \phi + \sin^2 \phi \, d\phi \sqrt{A^2 + B^2} \cdot \dfrac{\mp B}{\sqrt{A^2 + B^2}}}{\sqrt{A^2 \sin^2 \phi \, (d\phi^2 + d\theta^2) + 2AB \sin \phi \cos \phi \, d\phi \, d\theta + B^2 d\phi^2}}$$

$$= \mp \frac{B \, d\phi + A \sin \phi \cos \phi \, d\theta}{\sqrt{A^2 \sin^2 \phi \, (d\phi^2 + d\theta^2) + 2AB \sin \phi \cos \phi \, d\phi \, d\theta + B^2 d\phi^2}} \ldots\ldots \quad (15.)$$

The values of the differential coefficients derived from (8) being inserted in (15), give the cosine of the radius of curvature of (8) at the point $\phi\,\theta$. Inserted in (14), they give the polar distance of the centre of curvature: and inserted in (16) next following, they give the longitude of the centre of curvature.

Resume (12), expand it, and arrange according to $\sin\kappa$ and $\cos\kappa$: then we have

$$\cos\kappa\,(A\cos\theta + B\sin\theta) + \sin\kappa\,(A\sin\theta - B\cos\theta) = 0,$$

$$\text{or } \tan\kappa = \frac{A\cos\theta + B\sin\theta}{B\cos\theta + A\sin\theta} \quad \ldots\ldots\ldots\ldots\ldots \quad (16.)$$

The magnitude and position of the circle of curvature is hence determined when we insert from the given equation (8), the values of the differential coefficients in the forms above found.

It will be remarked, that these results suppose both variables to be dependent upon some third variable; that is, they are such as that the first differential of either does not become constant. We may, in general, then, very greatly simplify these expressions by taking either $d^2\phi$ or $d^2\theta$ equal to zero, inasmuch as B will then become either

$$\frac{\cos\phi\,d\theta\,d\phi + \sin\phi\,d^2\theta}{\sin\phi\,d\phi^2}, \text{ or } \frac{\cos\phi\,d\theta\,d\phi^2 - d^2\phi\,d\theta\sin\phi}{\sin\phi\,d\phi^2}.$$

It is unnecessary to pursue this subject farther here, or we might effect a considerable simplification by the reduction of the denominator of (15) to another form. We shall, therefore, finally, make one remark concerning the evolute or locus of the centre of curvature.

This may be considered either as the locus of the centre of curvature, or as the locus of the intersections of the consecutive normals.

Let $\qquad f(\phi\,\theta) = 0 \ldots\ldots\ldots\ldots\ldots (17.)$

be the curve whose evolute is sought.

Then if between (14, 16, 17), we eliminate ϕ and θ, we shall have an equation involving only κ and λ with constants, which will be that of the evolute required.

If we consider it as the locus of the intersections of the consecutive normals, we must proceed thus:—

Let $\sin \overline{\theta_{,} - \theta} \dfrac{d\phi_{,}}{d\theta_{,}} + \cos \overline{\theta_{,} - \theta} \sin \phi_{,} \cos \phi_{,} + \cos^2 \phi_{,} \tan \phi = 0$ ... (18.)

be the equation of the normal (*vid.* XL. 5.)

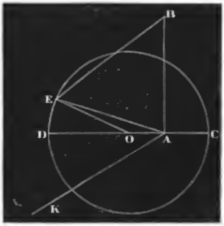
This by (17) is rendered a function of $\phi_{,}$ or $\theta_{,}$ only; and hence if we differentiate it relative to $\phi_{,}$ or $\theta_{,}$ (as the case may require or best admit of), and between these results, eliminate that quantity, we shall have an equation between ϕ θ and the constants of (17), which will be that of the evolute.

The involute is also found in a manner precisely analogous to that employed in the investigation of plane curves; and requires no remark, as there is no principle employed in the process when performed on the sphere, which is materially even a variation of that which is employed in the investigation of plane involutes.

XLV.

The Polar Equation of a Cone of the Second Degree and its trace upon the surface of a Concentric Sphere.

Let B be the vertex of a cone of the second degree, and CED any circular section of it. Let the perpendicular BA fall from B upon CED.



Refer the circle to the polar co-ordinates A and AK. Put $DO = a$, $AB = c$, $AO = b$; then $AC = a - c$, $AD = a + c$. Put $EA = r$, $EAK = \theta$,

and $DAK = \alpha$. Then we shall have

$$r^2 = a^2 - 2ab\cos\overline{\theta - \alpha} + b^2 \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (1.)$$

Now join EB, and denote the angle EBA by ϕ: and since EAB is a right angle, we shall have

$$r = c\tan\phi \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

Eliminating r from (1), (2), we have the polar equation of the cone, referred to origin B, and lines AB, AK; viz.

$$c^2\tan^2\phi = a^2 - 2ab\cos\overline{\theta - \alpha} + b^2 \quad \ldots\ldots\ldots\ldots \quad (3.)$$

Also since all parallel sections of the cone whose vertex is B and base CED are similar, we have the following quantities constant in all the sections; viz.

$$\frac{a^2 + b^2}{c^2} \quad \text{and} \quad \frac{ab}{c^2}.$$

Put these equal to $\tan^2\alpha_,$ and $\tan^2\alpha_{,,}$: then the equation of the cone becomes

$$\tan^2\phi = \tan^2\alpha_, - 2\tan^2\alpha_{,,}\cos\overline{\theta - \alpha}, \quad \text{or}$$

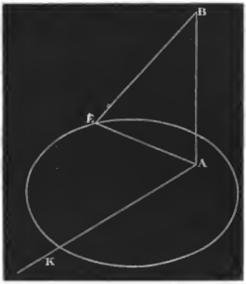
$$\cos\overline{\theta - \alpha} = \frac{\tan^2\alpha_, - \tan^2\phi}{2\tan^2\alpha_{,,}} = \tfrac{1}{2}\cot^2\alpha_{,,}(\tan^2\alpha_, - \tan^2\phi) \quad \ldots\ldots\ldots \quad (4.)$$

Now since every cone of the second degree admits of circular sections, it is quite obvious that all the equations of the cone, whatever form they may assume under their respective circumstances of data, may be *transformed* into (4). Nevertheless, it is often extremely troublesome to effect these transformations, and almost always unnecessary; and we shall at once seek the most general form that the equation of the cone referred to its vertex can assume. The method is extremely simple, and precisely similar to that which we have already employed.

We may take as the equation of a line of the second order referred to polar co-ordinates situated in its own plane,

$$\begin{aligned} r^2(\tan^2\alpha\cos^2\theta + 2\tan\alpha\tan\beta\tan\gamma\cos\theta\sin\theta + \tan^2\beta\sin^2\theta) & \\ + 2a_, r\tan\alpha_,\cos\theta & \\ + 2b_, r\tan\beta_,\sin\theta & \\ + d^2\tan\gamma^2 = 0 & \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (5.) \end{aligned}$$

Let the annexed figure referred to A and AB be that which is expressed



by (5); A being the foot of the perpendicular from the summit B of the cone. Put $BA = b$: then if $EBA = \phi$, we have

$$AE = r = b \tan\phi \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (5.)$$

By eliminating r we have

$$b^2 \tan^2\phi \{\tan^2\alpha \cos^2\theta + 2 \tan\alpha \tan\beta \tan\gamma \sin\theta \cos\theta + \tan^2\beta \sin^2\theta\}$$

$$+ 2b \tan\phi \{a_{,} \tan\alpha \cos\theta + b_{,} \tan\beta \sin\theta\} + d^2 \tan^2\delta = 0 \quad \ldots\ldots\ldots \quad (6.)$$

If we multiply this by $\cos^2\phi$, we may write it under the form

$$0 = A \cos^2\phi + 2 \sin\phi \cos\phi' (E \cos\theta + F \sin\theta)$$
$$+ \sin^2\phi (G \cos^2\theta + 2 H \sin\theta \cos\theta + \kappa \sin^2\theta) \quad \ldots\ldots\ldots \quad (7.)$$

This, with the exception of the first side of the equation, is the same as (XIV. 4.); and hence, as this difference is merely that of *less generality*, we learn at once that our last equation (7) designates a conic section on the surface of the sphere; or, in other words, that *the intersection of a cone of the second degree with a concentric sphere gives a spherical conic section such as was formerly defined* (XXIV.), in analogy to a very common mode of defining the plane sections of the cone. The analogy we see, then, is perfect between the two.

It may here, however, be suggested, that as the present equation (7) is

less general than (XIV. 5.), it is possible to annex some other condition (one or more, as subsequent investigation may shew to be admissible), to those involved in our present article; or, in other words, that the section of the cone by a sphere under some less confined relation may still give such a spherical conic section as we have previously defined. Comparing the present with the case where the sphere becomes a plane, we see the one case is given by a concentric sphere and the other a sphere whose centre is infinitely distant. It is natural to ask, if the analogy is confined to these two cases, or may the centre of the cutting sphere be situated any where in space? We have not room to enter upon this inquiry; but we may just state that the last is not the case. It is in one specific point, determined by the magnitude of the constant on the left side of the equation.

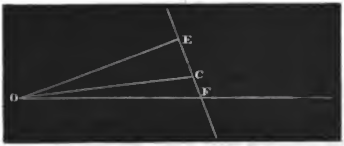
XLVI.

It will readily appear, that by taking the radius of the sphere infinite, the modifications which the spherical equations undergo, ought to have some analogy to the equations of plane curves. We cannot, it is true, always infer the plane from the spherical equation, much less the spherical from the plane, when there is an analogy between the genesis. The discussion of this curious question is omitted here,—not as irrelevant, but as too long to be admitted on the present occasion. There is, however, in general, a considerable degree of analogy between the two; and to shew this in perhaps the most important case that can occur, I shall investigate, *ab initio, the polar equation of a straight line,* for the purpose of comparison with the polar equation of a great circle given in my former paper.

On the Polar Equation of a Straight Line.

1. *The equation of a straight line,* the origin of the polar angle being OC, and the origin of the radius-vector being O, is thus found:

Let EC be the straight line whose equation is sought, and OE be a per-



pendicular from O upon it. Let F be a point in the line, and put

$$\text{OE} = a;$$
$$\text{OF} = r;$$
$$\text{COE} = \beta;$$
$$\text{and COF} = \theta;$$

Then $\quad \text{OE} = \text{OF} \cos \text{EOF}$

$$\text{or} \quad r = a \sec (\theta - \beta) \quad \ldots\ldots\ldots\ldots \quad (1.)$$

which is the equation sought.

It may also be written,

$$\cos (\theta - \beta) = \frac{a}{r} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

2. *To find the equation of a straight line through two given points.*

Let $r_{\prime}\,\theta_{\prime}$ and $r_{\prime\prime}\,\theta_{\prime\prime}$ be the points: then inserting these in (2) we get

$$\left.\begin{aligned} \cos (\theta_{\prime} - \beta) &= \frac{a}{r_{\prime}} \\ \cos (\theta_{\prime\prime} - \beta) &= \frac{a}{r_{\prime\prime}} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (3.)$$

From which

$$\frac{r_{\prime\prime}}{r_{\prime}} = \frac{\cos\beta\cos\theta_{\prime} + \sin\beta\sin\theta_{\prime}}{\cos\beta\cos\theta_{\prime\prime} + \sin\beta\sin\theta_{\prime\prime}} = \frac{\cos\theta_{\prime} + \tan\beta\sin\theta_{\prime}}{\cos\theta_{\prime\prime} + \tan\beta\sin\theta_{\prime\prime}}$$

$$\left.\begin{aligned} \text{or} \quad \tan\beta &= -\frac{r_{\prime}\cos\theta_{\prime} - r_{\prime\prime}\cos\theta_{\prime\prime}}{r\sin\theta_{\prime} - r_{\prime\prime}\sin\theta_{\prime\prime}} \\ \sin\beta &= \mp\frac{r_{\prime}\cos\theta_{\prime} - r_{\prime\prime}\cos\theta_{\prime\prime}}{\sqrt{r_{\prime}^2 - 2r_{\prime}r_{\prime\prime}\cos\overline{\theta_{\prime} - \theta_{\prime\prime}} + r_{\prime\prime}^2}} \\ \cos\beta &= \pm\frac{r_{\prime}\sin\theta_{\prime} - r_{\prime\prime}\sin\theta_{\prime\prime}}{\sqrt{r_{\prime}^2 - 2r_{\prime}r_{\prime\prime}\cos\overline{\theta_{\prime} - \theta_{\prime\prime}} + r_{\prime\prime}^2}} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (4.)$$

Again, to find $a_{,}$ we have from (3) the equations

$$\beta = \cos^{-1}\frac{a}{r_{,}} + 0,$$

$$\beta = \cos^{-1}\frac{a}{r_{,,}} + \theta_{,,}$$

or $$\cos^{-1}\frac{a}{r_{,}} - \cos^{-1}\frac{a}{r_{,,}} = \theta_{,,} - \theta_{,} \qquad (5.)$$

Take cosine-function of these, square, and transpose: which gives, after slight reduction,

$$a = \pm\frac{r_{,}r_{,,}\sin\overline{\theta_{,,}-\theta_{,}}}{\sqrt{r_{,}^2 - 2r_{,}r_{,,}\cos\overline{\theta_{,,}-\theta_{,}} + r_{,,}^2}} \qquad (6.)$$

Insert these values of β from (4) in equation (2), and we have

$$\frac{a}{r} = \cos\beta\cos\theta + \sin\beta\sin\theta$$

$$= \frac{\pm\cos\theta\,(r_{,}\sin\theta_{,} - r_{,,}\sin\theta_{,,}) \mp \sin\theta\,(r_{,}\cos\theta_{,} - r_{,,}\cos\theta_{,,})}{\sqrt{r_{,}^2 - 2r_{,}r_{,}\cos\overline{\theta_{,,}-\theta_{,}} + r_{,,}^2}} \qquad (7.)$$

and from equation (6) inserting the value of a, we find finally

$$\frac{r_{,}r_{,,}\sin\overline{\theta_{,,}-\theta_{,}}}{r} = \left\{\begin{array}{l}\pm\cos\theta\,(r_{,}\sin\theta_{,} - r_{,,}\sin\theta_{,,}) \\ \mp\sin\theta\,(r_{,}\cos\theta_{,} - r_{,,}\cos\theta_{,,})\end{array}\right\} \qquad (8.)$$

or by division,

$$\frac{1}{r} = \frac{r_{,}\sin\overline{\theta\mp\theta_{,}} - r_{,,}\sin\overline{\theta\mp\theta_{,,}}}{r_{,}r_{,,}\sin\overline{\theta_{,,}\mp\theta_{,}}} \qquad (9.)$$

If now we keep in mind that $\frac{1}{r}$, $\frac{1}{r_{,}}$, and $\frac{1}{r_{,,}}$ are the same things that $\cot\phi$, $\cot a$, and $\cot a_{,,}$ on the sphere become when the sphere is infinite, we get precisely the same result as in my paper, (IV. 10). That is

$$\frac{1}{r} = \frac{\frac{1}{r_{,,}}\sin\overline{\theta\mp\theta_{,}} - \frac{1}{r_{,}}\sin\overline{\theta\mp\theta_{,,}}}{\sin\overline{\theta_{,,}\mp\theta_{,}}} \qquad (10.)$$

is the form which may be considered as the extreme case of that result, the sphere becoming then infinite.

3. *To find the point of intersection of two given lines, referred to polar co-ordinates.*

Let them be

$$\left.\begin{aligned} r &= a_{\prime} \sec \overline{\theta - \beta_{\prime}} \\ r &= a_{\prime\prime} \sec \overline{\theta - \beta_{\prime\prime}} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{(11.)}$$

From which by equating we get,

$$a_{\prime} \cos \overline{\theta - \beta_{\prime}} = a_{\prime\prime} \cos \overline{\theta - \beta_{\prime\prime}},$$

or, by expansion, etc.

$$\tan \theta = -\frac{a_{\prime\prime} \sin \beta_{\prime} - a_{\prime} \sin \beta_{\prime\prime}}{a_{\prime\prime} \cos \beta_{\prime} - a_{\prime} \cos \beta_{\prime\prime}} \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{(12.)}$$

Again, from (11) we have

$$\cos \overline{\theta - \beta_{\prime}} = \frac{a_{\prime}}{r}$$

$$\cos \overline{\theta - \beta_{\prime\prime}} = \frac{a_{\prime\prime}}{r}$$

or by taking $\cos^{-1}$ of each equation and transposing

$$\overline{\beta_{\prime} - \beta_{\prime\prime}} = \cos^{-1} \frac{a_{\prime\prime}}{r} - \cos^{-1} \frac{a_{\prime}}{r}, \text{ or}$$

$$\cos \overline{\beta_{\prime} - \beta_{\prime\prime}} = \frac{a_{\prime} a_{\prime\prime}}{r^2} \pm \sqrt{\left(1 - \frac{a_{\prime\prime}^2}{r^2}\right)\left(1 - \frac{a_{\prime}^2}{r^2}\right)}$$

Transposing, squaring, etc. gives

$$a_{\prime}^2 - 2 a_{\prime} a_{\prime\prime} \cos \overline{\beta_{\prime} - \beta_{\prime\prime}} + a_{\prime\prime}^2 = r^2 \sin^2 \overline{\beta_{\prime} - \beta_{\prime\prime}}$$

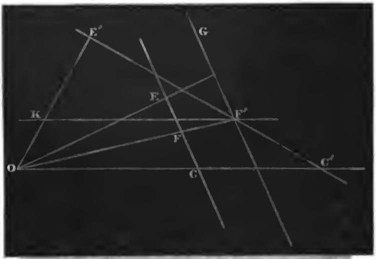
$$\text{or} \quad \frac{1}{r} = \pm \frac{\sin \overline{\beta_{\prime} - \beta_{\prime\prime}}}{\sqrt{a_{\prime}^2 - 2 a_{\prime} a_{\prime\prime} \cos \overline{\beta_{\prime} - \beta_{\prime\prime}} + a_{\prime\prime}^2}} \quad \ldots\ldots\ldots\ldots\ldots\ldots \text{(13.)}$$

which may be compared with (VI. 6) of my former paper *.

* It may here be remarked, that the varied form of the equation referred to was accidentally omitted in printing that article. Divide all the terms by $\cot \lambda_{\prime}$, $\cot \lambda_{\prime\prime}$ and we get for (VI. 7) the following:

$$\cot \varphi = \frac{\pm \tan \lambda_{\prime} \tan \lambda_{\prime\prime} \sin \overline{\varkappa_{\prime\prime} - \varkappa_{\prime}}}{\sqrt{\tan \lambda_{\prime} - 2 \tan \lambda_{\prime} \tan \lambda_{\prime\prime} \cos \overline{\varkappa_{\prime\prime} - \varkappa_{\prime}} + \tan^2 \lambda_{\prime\prime}}} \quad \ldots\ldots\ldots\ldots \text{(VI. 7.)}$$

4. *To find the equation of a line which passes through a given point $r_, \theta_,$ and makes a given angle ϵ with a given straight line $r = a \sec \overline{\theta - \beta}$.*



Let EC be the given line, F′ the given point, and E′F′C′ the line sought. Put its equation in the form,

$$r = c \sec \overline{\theta - \gamma} \quad \text{.......} \quad (14.)$$

But it passes through $r_, \theta_,$, and hence

$$r_, = c \sec \overline{\theta_, - \gamma}, \text{ or } c = r_, \cos \overline{\theta_, - \gamma} \quad \text{.......} \quad (15.)$$

Also, $\gamma = \text{E}'\text{OC}' = \beta + \epsilon$, (where $\text{E} = \text{E}'\text{F}'\text{G}$), and hence

$$r = r_, \cos (\theta_, - \overline{\beta + \epsilon}) \sec (\theta - \overline{\beta + \epsilon}) \quad \text{.......} \quad (16.)$$

which is the required equation.

Let $\epsilon = 0$, or the lines be parallel: then

$$r = r_, \cos (\theta_, - \beta) \sec (\theta - \beta) \quad \text{.......} \quad (17.)$$

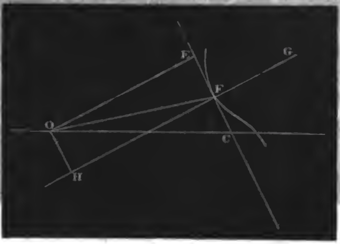
Let $\epsilon = \frac{\pi}{2}$, or the lines be perpendicular: then

$$r = r_, \sin \overline{\theta_, - \beta} \operatorname{cosec} \overline{\theta - \beta} \quad \text{.......} \quad (18.)$$

It will be altogether unnecessary to multiply examples of this analogy between plane and spherical equations, as the method of forming all such

equations is sufficiently obvious. We shall, therefore, here only add the equations of the tangent and normal of a polar plane curve in terms of the differential coefficients and the co-ordinates of the points concerned.

5. *To find the tangent to a polar curve.*



The equation of EC is

$$r = a \sec \overline{\theta - \beta} \quad \text{.......... ....} \quad (19.)$$

Now the angle OFE is denoted by

$$\tan^{-1} - \frac{r_{,} d\theta_{,}}{d r_{,}}, \quad \text{and}$$

$$\mathrm{OE} = r_{,} \sin \tan^{-1} - \frac{r_{,} d\theta_{,}}{d r_{,}} = a \quad \text{.................} \quad (20.)$$

Again $$\mathrm{EOC} = \frac{\pi}{2} - \mathrm{EFO} + \theta_{,} = \beta$$

or $$\beta = \frac{\pi}{2} - \tan^{-1} \frac{-r_{,} d\theta_{,}}{d r_{,}} + \theta_{,} \quad \text{..........................} \quad (21.)$$

Expanding (19) we have

$$\frac{a}{r} = \cos\theta \cos\beta + \sin\theta \sin\beta$$

$$= \cos\theta \sin\left(\tan^{-1} \frac{-r_{,} d\theta_{,}}{d r_{,}} - \theta_{,}\right) + \sin\theta \cos\left(\tan^{-1} \frac{-r_{,} d\theta_{,}}{d r_{,}} - \theta_{,}\right)$$

or writing for $\sin \tan^{-1} \frac{-r_{,} d\theta_{,}}{dr_{,}}$ and $\cos \tan^{-1} \frac{-r_{,} d\theta_{,}}{dr_{,}}$ their values we get

$$\frac{a}{r} = \frac{\cos\theta\,\{-r_{,}d\theta_{,}\cos\theta_{,} - dr_{,}\sin\theta_{,}\} + \sin\theta\,\{dr_{,}\cos\theta_{,} - r_{,}d\theta_{,}\sin\theta_{,}\}}{\sqrt{r_{,}^{2}d\theta_{,}^{2} + dr_{,}^{2}}} \quad \ldots (22.)$$

$$= -\frac{r_{,}d\theta_{,}\cos\overline{\theta - \theta_{,}} - dr_{,}\sin\overline{\theta - \theta_{,}}}{\sqrt{r_{,}^{2}d\theta_{,}^{2} + dr_{,}^{2}}} \quad \ldots\ldots (23.)$$

Also from (20) we have

$$a = \frac{-r_{,}^{2}d\theta_{,}}{\sqrt{r_{,}^{2}d\theta_{,}^{2} + dr_{,}^{2}}} \quad \ldots\ldots (24.)$$

which inserted in (23) gives

$$\frac{1}{r} = \frac{r_{,}d\theta_{,}\cos\overline{\theta - \theta_{,}} - dr_{,}\sin\overline{\theta - \theta_{,}}}{r_{,}^{2}d\theta_{,}} \quad \ldots\ldots (25.)$$

$$= \frac{\cos\overline{\theta - \theta_{,}}}{r_{,}} - \frac{\sin\overline{\theta - \theta_{,}}}{r_{,}} \cdot \frac{dr_{,}}{r_{,}d\theta} \quad \ldots\ldots (26.)$$

6. *To find the equation of the normal.*

Here the angle HOF is denoted by $\tan^{-1}\frac{dr_{,}}{r_{,}d\theta_{,}}$, or

$$\beta = \frac{\pi}{2} - \tan^{-1}\frac{dr_{,}}{r_{,}d\theta_{,}} + \theta_{,} \quad \ldots\ldots (27.)$$

Resuming the general form of (19) expanded, viz.

$$\frac{a}{r} = \cos\beta\cos\theta + \sin\beta\sin\theta,$$

we have, by substitution,

$$\frac{a}{r} = \cos\theta\sin\left(\tan^{-1}\frac{dr_{,}}{r_{,}d\theta} - \theta_{,}\right) + \sin\theta\cos\left(\tan^{-1}\frac{dr_{,}}{r_{,}d\theta_{,}} - \theta_{,}\right)$$

and writing for $\sin\tan^{-1}\frac{dr_{,}}{r_{,}d\theta_{,}}$ and $\cos\tan^{-1}\frac{dr_{,}}{r_{,}d\theta_{,}}$ these values, we obtain

$$\frac{a}{r} = \frac{\cos\theta\,\{\cos\theta_{,}\,dr_{,} - \sin\theta_{,}\,r_{,}d\theta_{,}\} + \sin\theta\,\{\cos\theta_{,}\,r_{,}d\theta_{,} + \sin\theta_{,}\,dr_{,}\}}{\sqrt{r_{,}^{2}d\theta_{,}^{2} + dr_{,}^{2}}} \quad \ldots (28.)$$

that is,

$$\frac{a}{r} = \frac{\cos \overline{\theta - \theta_,}\, dr_, + \sin \overline{\theta - \theta_,}\, r d\theta_,}{\sqrt{r_,^2 d\theta_,^2 + dr_,^2}} \quad \ldots\ldots (29.)$$

or, giving to a its value, viz.

$$a = r_, \sin \tan^{-1} \frac{dr_,}{r_, d\theta_,}$$

$$= r_, \frac{dr_,}{\sqrt{r_,^2 d\theta_,^2 + dr_,^2}} \quad \ldots\ldots (30.)$$

we shall have (29.) converted into

$$\frac{1}{r} = \frac{\cos \overline{\theta - \theta_,}}{r_,} + \frac{\sin \overline{\theta - \theta_,}}{r_,} \cdot \frac{r_, d\theta_,}{dr_,} \quad \ldots\ldots (31.)$$

Whether the formulæ (26.) and (31.) may ultimately assist us to a greater extent than the methods now in use, in investigating the general affections of polar curves, is not a proper subject of discussion for this note; but it may at some future period be inquired into more carefully. My own employment of these formulæ in a considerably varied number of trials, made upon curves taken perfectly at hazard, leads me to think that they will be found of great value; but I have not entered sufficiently into a systematic examination of the system to be able to say that there might not be obstacles in the way of its universal application, that have not presented themselves in any of the particular cases which I have happened to select for experiment. I think it, however, highly probable, that (as I have found to exist on the sphere) there will be some cases where one method, and others where the other method, will be most advantageous; and as, on the sphere, there does not appear to be any method of judging *à priori* which of the methods will be most applicable, so also *in plano,* we shall be able to find no test for deciding the question but actual experiment on the particular cases under consideration. If nothing shall be gained in the way of *general principles,* yet it will be of some advantage to be put in possession of an alternative method of investigation, that offers considerable prospect of enabling us to analyse the curve, when the common method shall, in that particular case, either fail altogether, or else, by the complexity of the result, be thought less elegant than the taste of the geometer may lead him to desire.

CORRECTIONS AND ADDITIONS OF MY FORMER PAPER.

From oversight in transcribing for the press, or in correcting the proofs, a few errors have crept into that paper, which I here take the opportunity to indicate. I shall also supply certain defects in that paper, which were almost inseparable from the circumstances under which it was written.

P. 263, last word, *for* im- *read* ex-

— 265, l. 21, *for* occasional inquiries *read* unconnected inquiries

— 267, l. 15, *for* where greatest *read* where the greatest

— 267, l. 5 from bottom, *for* great force *read* equal force

— 268, l. 14, bott. This is a mistake. James Bernoulli in another place had noticed the identity of the methods, as I had mentioned in the foot-note of p. 308.

— 274, *Cor.* 6. *for* $\lambda = \frac{\pi}{2}$. *read* $\phi = \frac{\pi}{2}$.

— 279, Eq. 16. might also be changed into a form more convenient for some purposes by a process similar to that in (iv. 10.)

— 280, If we divide all the terms of equation (6.) by $\cot \lambda_{,} \cot \lambda_{,,}$ it will take a more convenient form,

$$\cot \phi = \frac{\pm \tan \lambda_{,} \tan \lambda_{,,} \sin \overline{\kappa_{,,} - \kappa_{,}}}{\sqrt{\tan^2 \lambda_{,} - 2 \tan \lambda_{,} \tan \lambda_{,,} \cos \overline{\kappa_{,,} - \kappa_{,}} + \tan^2 \lambda_{,,}}} \ldots\ldots\ldots\ldots (7.)$$

— 281, l. 10, *for* $\frac{\pi}{2} + \kappa = \widehat{FD}$, *read* $\frac{\pi}{2} + \kappa = \widehat{ED}$,

— — 14, For $\cot \overline{\kappa - \beta_{,}}$ in the denominator, *read* $-\cot \overline{\kappa - \beta_{,}}$

We may remark, that the double sign of eq. (5.) is purely from the radical, as the division which produces it was

$$\frac{-\tan \lambda \cos \overline{\beta_{,} - \kappa}}{-\cot \overline{\kappa - \beta_{,}}} \times \pm \sqrt{\frac{1}{1 + \tan^2 \lambda \cos^2 \overline{\beta - \kappa_{,}}}} \quad \text{and } \cos \overline{\beta_{,} - \kappa} = \cos \overline{\kappa - \beta_{,}}$$

P. 284, Eq. (7.) may be modified in the same manner as (IV. 10.) was modified.

— 285, Eq. (10). This may be written

$$\cot \phi = \mp \frac{\cot \alpha, \sin \overline{\beta,-\theta} - \cot \lambda, \sin \overline{\kappa,-\theta}}{\sin \overline{\beta,-\kappa}}$$

and thus agree with a circle passing through $\alpha, \beta,$ and $\kappa, \lambda,$; as it should do (see IV. 10.) the perpendicular to any circle passing through its spherical centre.

— 288, Prop. IX. In equation (4.) put

[and from equation (2.), $\cos \overline{\beta-\kappa} = -\cot \alpha \cot \lambda$...............(4.)]

Ib. In passing from the first to the second value of $\cos \kappa$, we have

$$\cos \kappa = -\cos \beta \cot \alpha \cot \lambda + \sin \beta \sqrt{1 - \cot^2 \alpha \cot^2 \lambda}$$

$$= \frac{-\cos \beta \cot \alpha \sin \alpha \sin \epsilon}{\pm \sqrt{1 - \sin^2 \alpha \sin^2 \epsilon}} + \frac{\pm \sin \beta \sqrt{1 - \sin^2 \alpha \sin^2 \epsilon - \cot^2 \alpha \sin^2 \alpha \sin^2 \epsilon}}{\pm \sqrt{1 - \sin^2 \alpha \sin^2 \epsilon}}$$

$$= \frac{-\cos \beta \cos \alpha \sin \epsilon + \sin \beta \times (\pm \cos \epsilon)}{\pm \sqrt{1 - \sin^2 \alpha \sin^2 \epsilon}} \quad \text{.......................................} (7.)$$

Now, in this, the sign of the denominator is $\pm$, but *independent of the sign of the radical in the numerator of the fraction*, and hence the double sign $\pm$ in the numerator ought to be retained in any result that flows from our inquiries, though the denominator itself should be eliminated from the expression. The same rule is true for the expression $\sin \kappa$, which gives

$$\sin \kappa = \frac{-\sin \beta \cos \alpha \sin \epsilon - \cos \beta \times (\pm \cos \epsilon)}{\pm \sqrt{1 - \sin^2 \alpha \sin^2 \epsilon}} \quad \text{..........................} (8.)$$

The equation of the circle then becomes

$$\cot \phi = -\frac{\pm \sqrt{1 - \sin^2 \epsilon \sin^2 \alpha}}{\sin \alpha \sin \epsilon} \times \left\{ \begin{array}{l} \cos \theta \left\{ \dfrac{-\cos \beta \cos \alpha \sin \epsilon + \sin \beta (\pm \cos \epsilon)}{\pm \sqrt{1 - \sin^2 \epsilon \sin^2 \alpha}} \right\} \\ + \sin \theta \left\{ \dfrac{-\sin \beta \cos \alpha \sin \epsilon - \cos \beta (\pm \cos \epsilon)}{\pm \sqrt{1 - \sin^2 \epsilon \sin^2 \alpha}} \right\} \end{array} \right\}$$

from which the $\pm \sqrt{1 - \sin^2 \epsilon \sin^2 \alpha}$, which is common to both numerator and denominator, vanishes of itself, its sign included. The equation then reduces itself to

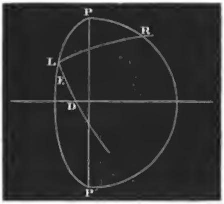
$$\cot\phi = -\operatorname{cosec}\alpha\operatorname{cosec}\epsilon\left\{\begin{array}{l}\cos\theta\,(-\cos\beta\cos\alpha\sin\epsilon\pm\sin\beta\cos\epsilon)\\ +\sin\theta\,(-\sin\beta\cos\alpha\sin\epsilon\mp\cos\beta\cos\epsilon)\end{array}\right\}\ldots\ldots(9.$$

This expression must be substituted for the (9.) of that paper. In that paper, I had taken only the $+$ in deriving equation (7.); and in obtaining (8.) I had employed the equation

$$\sin\kappa=\sqrt{1-\cos^2\kappa}\quad\text{instead of}\quad\sin\kappa=\pm\sqrt{1-\cos^2\kappa}.$$

The formula thus obtained was incomplete in an analytical point of view; and the complete analytical solution was redundant in its application to the confined geometrical aspect under which I then viewed it. Unfortunately, too, the particular algebraical case, and the particular geometrical one which I separately considered, were not those which corresponded to one another; and consequently, by attaching them together, the result which thus presented itself was not merely incomplete, but it was erroneous. No consequences were deduced from it, however, in that paper; and hence no inconvenience has arisen, except to myself, in being obliged to recalculate those expressions into which this equation entered. The rule for the signs, then, is this:—

When the angle ϵ is estimated from the branch of the meridian LP *between the given point and the pole of reference, the sign of* $\cos\epsilon$ *is* $+$; *and when from the opposite branch* LP′, *then* $-$ *is to be employed.*



Thus, if the angle ϵ be P′LD, *the sign* $+$ *or* $-$ *is to be used according as the pole of reference is* P′ *or* P.

The proof of this is very simple.

We see at once, from the manner in which ϵ and β enter into the equation, that the *sign* of $\cos\epsilon$ (since ϵ and β are themselves mutually independent) must be totally independent of the value of β, that is, of the origin of angular ordinates; and, therefore, as it will considerably simplify our investigation, we shall take $\beta = 0$. This gives

$$\cot\phi = -\operatorname{cosec}\epsilon \operatorname{cosec}\alpha \{\cos\theta(-\cos\alpha\sin\epsilon) + \sin\theta(\mp\cos\epsilon)\} \text{ ....(10.)}$$

In this we have
$$\cot\kappa = \frac{-\cos\alpha\sin\epsilon}{\mp\cos\epsilon}$$
$$= \pm\cos\alpha\tan\epsilon \text{ ....(11.)}$$

Now, by the quadrantal triangle PLR, we have

$$\cot \mathrm{LPR} = \cot\kappa = -\cos\alpha\tan\epsilon \text{ ....(12.)}$$

And by the quadrantal triangle P′LR, we have

$$\cot\kappa = +\cos\alpha\tan\epsilon \text{ ....(13.)}$$

We see, therefore, that in the case of P and the origin of ϵ being on the same or on different sides of L, the upper or lower sign of (11.) is to be taken, in conformity respectively with (13.) and (12.), which is the rule given above.

The equation of LD, then, as referred to P and P′ respectively, is

$$\cot\phi = -\operatorname{cosec}\alpha\operatorname{cosec}\epsilon\left\{\begin{matrix}-\cos\theta\,(\sin\beta\cos\epsilon + \cos\beta\cos\alpha\sin\epsilon)\\ +\sin\theta\,(\cos\beta\cos\epsilon - \sin\beta\cos\alpha\sin\epsilon)\end{matrix}\right\} \text{......(14.)}$$

$$\cot\phi = -\operatorname{cosec}\alpha\operatorname{cosec}\epsilon\left\{\begin{matrix}+\cos\theta\,(\sin\beta\cos\epsilon - \cos\beta\cos\alpha\sin\epsilon)\\ -\sin\theta\,(\cos\beta\cos\epsilon + \sin\beta\cos\alpha\sin\epsilon)\end{matrix}\right\} \text{......(15.)}$$

It might be thought, however, for the sake of generality in the subsequent investigation, to be as well to retain the double form given in (9.): though, in many cases, our purpose may be fully answered by the adoption of either one or other of its components (14.) or (15.), merely taking care to keep clearly in view the connection between the pole employed and the branch of the meridian from which ϵ is measured. Except contrarily expressed, I shall use the pole P and branch P′L, from its being that adapted to the figures I have most commonly used. This requires the lower sign —, and (14.) is the equation of the circle adapted to that assumption.

P. 291, Eq. 16. In the numerators, *for* $2E^2 - D^2$ and $2D^2 - E^2$ *read* $2(E^2 - D^2)^2$ and $2(D^2 - E^2)^2$
In the second equation, *for* sin *read* cos
In the denominator, *for* E^1 *read* E^2

— 292, l. 1, *for* (4.) from (3.) *read* (4.) from (2.)

— 293, In equation (14.), coefficient of $\sin\phi$, *for* $\sin\alpha \sin\beta - \sin\alpha_, \sin\beta_,$ *read* $\sin\alpha \sin\beta - \sin\alpha_, \sin\beta_{,,}$ in the *upper line* of the expression; and in the *lower*,—*for* $\sin\beta$, $\sin\beta_,$, $\sin\beta_{,,}$ wherever they occur, *read* $\cos\beta$, $\cos\beta_,$, $\cos\beta_{,,}$ respectively.

— 296, l. 4, Should be marked ......(7.)

— 299, XVI. the first equation should be

$$L = \int \sqrt{d\theta^2 \sin^2\phi + d\phi^2} + C$$

and the $d\phi$ in the last line should be $d\phi^2$

— 301, The equation expressing the value of A should have been numbered ......(6.)

— 304, l. 7. & 9, *for* commensurable *and* incommensurable *read* rational *and* irrational *respectively.*

— — l. 12, *for* the comma at the word functions *read* a full point.

— 305, l. 2, *for* the factor without the radical, viz. $d\left(\frac{\phi}{2} - \phi\right)$. *read* $d\left(\frac{\pi}{2} - \phi\right)$.

— — l. 6, from bottom, *for* $A\,2 = \pi - 4$ *read* $A = 2\pi - 4$

— 307, l. 5, *for* $\frac{m}{n}\sin\phi$ *read* $\sin\frac{m}{n}\phi$

— 308, l. 2, from bott. *for* $\sin\phi = \theta$ *read* $\sin\phi = \cos\theta$

— 311, l. 8, *for* quadrantal lines *read* quadrantal lunes

— — l. 12, *dele* to, *the second word of the line.*

I may here remark, that, in the figures there and elsewhere given, no attempt at the accuracy of instrumental construction has been made. It has been thought quite sufficient to convey a general notion of the course of the several curves, as projected upon a plane.

— 313, *dele* = from the end of the two intermediate values of $\cot\frac{E}{2}$

P. 315, l. 17, *for* (4.) *read* (5.)

— — l. 28, *for* upon in the *read* upon it in the

— 317, l. 10, *after* than a, *insert* and $i + a \angle \pi$

— — l. 12, for n read x and for (sin *read* sin (

— 318, l. 12, *for* uniformity *read* conformity

Ib. *Note.* I have attributed that question erroneously. It was not proposed by Professor WALLACE, but by Professor LOWRY, and I learn from my valued friend that he had not entertained any idea of treating it in the manner developed in these papers. My mistake arose from its being proposed *anonymously*, and from my accidentally interchanging in my own mind the signatures used by those distinguished geometers.

— 322, l. 16, *In* would not then *dele* not

— — — 19, *for* to search for—some *read* to search—for some

— 323, l. 16, *for* interesting amusement *read* instructive employment

— 324, l. 8, *for* were *read* was

— 325, l. 14, *for* $\overline{R \pm r}$ or $\overline{R - r}$ *read* $\overline{R + r}$ or $\overline{R - r}$

— 327, For *From equation* (13.) read *From equation* (12.)
From equation (14.) read *From equation* 13.)

— 333, l. 8, *for* triangle is in reality *read* triangle which he employs is in reality; and *for* the, at the end of the same sentence, *read* his

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ON THE LOXODROME.

(Vid. sect. XXX.—XXXIV.)

The *mathematical continuousness* of the ship's course in returning to the same place without changing her rhumb, it has been objected by a learned friend, was not satisfactorily established in my former paper. He

rather looks upon them as two distinct curves, whose ultimate tangents make an angle of 180° with each other. Perhaps my mode of considering it might justify such an objection—that they were two distinct curves, whose generating azimuths were the supplements of each other, and proceeding contrary ways from the meridian of reference; and, therefore, it becomes desirable to examine the question a little more closely. This I propose to do in the present note.

The differential equations in XXXI., viz. $d\theta \cot a = d\phi \operatorname{cosec} \phi$ might be written

$$\pm \cot a \, d\theta = \pm \frac{d\phi}{\sin\phi} = \frac{d\phi}{\sin(\pm\phi)}$$

and its integral will therefore be

$$\log\tan\left(\pm\frac{\phi}{2}\right) = \cot a \, . \, \theta + \text{const} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (3')$$

or since, as is found in the passage quoted, the const = 0, and

$$\log\tan\left(\pm\frac{\phi}{2}\right) = \theta \cot a \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (4')$$

This expresses two equal and symmetrical branches of the same curve referred to the same polar co-ordinates,—in short, the points which geographers denominate the *Periœci;* and being included in the same equation, constitute mathematically continuous branches of one curve, and not merely tangential branches of two separate curves. The validity of the inference is hence fully established, for these two numbers are those which, in my former paper, were shewn to meet in the pole; and hence the ship would return to the same point after pursuing such a course as was there laid down.

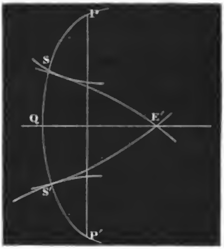
I was desirous of inserting here some further properties of the loxodrome, especially the analogues to James Bernoulli's properties of the logarithmic spiral*: but the space which I can allow myself does not allow me to do so, especially as I wish to notice here a new curve, having certain remark-

* Opp. tom ii. p. 491.

able relations to the loxodrome. I mean the equi-subtangential curve on the sphere, or as, for reasons which will be at once apparent, I shall denominate it, the Spherical Logarithmic Curve. Even this can be but slightly noticed here, and that merely to ascertain its general form, leaving its involute, evolute, and locus of intersection of perpendicular upon the tangent, altogether unnoticed. I also wished to add some speculations respecting the circumstances of the curve when the sphere becomes infinite; but these must also be deferred till a future period, when I can complete my paper on the singular points, etc. of Spherical Loci.

On the Spherical Logarithmic.

1. The subtangent of a certain curve intercepted on the equator by the current meridian is constant,—What is the nature of the curve?



Let QE $= a$ be the given subtangent: then by (XVI. 2) we have

$$\tan \mathrm{ESQ} = \frac{d\,\theta \sin \phi}{d\,\phi}, \text{ or } \cot \mathrm{ESQ} = \frac{d\,\phi}{\sin \phi \, d\,\theta} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1$$

Also by right-angled triangles,

$$\cos \phi = \tan a \cot \mathrm{ESQ} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (2.)$$

From (1, 2) we have

$$\pm \cot a \, d\theta = \pm \frac{d\phi}{\sin\phi \cos\phi} = \pm \frac{2\,d\phi}{\sin 2\phi} \quad \text{.................} \quad (3.)$$

Or integrating, we get

$$\tan(\pm\phi) = + c\,\epsilon^{\cot a \,.\, \theta} \quad \text{..................................} \quad (4.)$$

This equation (4) divides itself into two parts, according as $+\phi$ or $-\phi$ is taken; and hence we have

$$\tan(+\phi) = + c_{,}\,\epsilon^{\cot a\,\theta} \quad \text{......................................} \quad (5.)$$

$$\tan(-\phi) = + c_{,,}\,\epsilon^{\cot a\,\theta} \quad \text{......................................} \quad (6.)$$

We shall consider these equations separately: and, *first*, to find the constants $c_{,}$ and $c_{,,}$.

To find $c_{,}$ put $\phi = \frac{\pi}{4}$; then $\tan\phi = 1$, and $\log \tan\phi = 0$, and therefore

$$\cot a \,.\, \theta + \text{const} = 0, \quad \text{or}$$

if we take that meridian where $\phi = \frac{\pi}{4}$, for the origin of θ, we shall have

$$c_{,} = 1.$$

and our equation (5) becomes

$$\tan\phi = \epsilon^{\cot a \,.\, \theta} \quad \text{...} \quad (5')$$

Again, take $\phi = \frac{3\pi}{4}$; then $\tan\phi = -1$, and $\log \tan(-\phi) = \log 1 = 0$.

And hence, as before, we have

$$c_{,,} = 1,$$

which converts (6) into

$$\tan(-\phi) = \epsilon^{\cot a\,\theta} \quad \text{...} \quad (6')$$

and the united equation (4) may be written

$$\tan(\pm\phi) = \epsilon^{\cot a} \quad \text{...} \quad (4')$$

We shall next proceed to investigate its course, and we shall find that (5′ 6′) represent two branches, not continuous, but symmetrical with respect to the equator.

2. Take (5′); viz. $\tan\phi = \epsilon^{\cot a\,\theta}$. Then,

($a_{,}$). Let $\phi = 0$; then $\tan\phi = 0$, $\log\tan\phi = -\frac{1}{0}$: and as $\epsilon > 1$, all its powers are greater than 1; and hence $\theta = -\frac{1}{0}$, or the curve winds round the pole *negatively* to infinity—*or the pole is an asymptote.*

($b_{,}$). Let $\phi = \frac{\pi}{2}$; then $\tan\phi = \frac{1}{0}$, and $\log\tan\phi = \frac{1}{0}$, or the equator is an asymptote to the positive branch of the curve.

($c_{,}$) At all intermediate positions, the value of θ is finite, and either positive or negative.

The branch of the curve, then, designated by (5′) winds round the pole as a spiral in the negative direction from the origin of θ, and continually approximates towards the equator on the positive side of the same origin; but never reaches either the pole or the equator during any assignable number of revolutions.

($d_{,}$). If $\phi > \frac{\pi}{2}$; then $\tan\phi = -p$, and $\log\tan\phi$ is imaginary; that is the *branch* denoted by (5′) does not reappear *beyond* the equator, or does not *cut* it after infinitely revolving round the sphere.

($e_{,}$). There is, however, a secondary branch involved in the same equation, for $\tan\phi = \tan(\pi + \phi)$; and hence (5′) is fulfilled by this latter system of values of ϕ as well as by the former. At this point, however, the subtangent a is changed from a to $a + \pi$, and the second curve is that which is traced out by the opposite extremity s' of the diameter passing through the current point s of the first curve. The second curve, or the conical branch, as I have elsewhere called it, *is not a solution of the same geometrical problem, though it is of the same algebraic equation.* They are not, strictly speaking, branches of the *same curve;* but they are solutions of the same problem, *if, instead of the datum being a given arc, it had been an arc*

whose tangent was given. The problem, as we have resolved it for the tangent, is in a form more general than it was proposed; and resembles those cases in common algebra where irrelevant answers are mixed up with those required by the peculiar terms of the question. The same is true of all the secondary branches of spherical curves that result from solving the questions for some trigonometrical functions of an arc instead of solving them for the arc itself;—viz. those which we have denominated cylindrical branches given by means of sines, cosines, versed-sines, secants, and cosecants, with the conical ones given by means of tangents and cotangents.

$(f_{,})$. To find the ultimate inclination of the tangent to the equator, we have, by NAPIER,

$$\frac{\sin a}{\sin\phi} = \cot \mathrm{QES}$$

Now when $\phi = \dfrac{\pi}{2}$ we have $\cot \mathrm{QES} = \dfrac{\sin a}{0} =$ infinity, or $\mathrm{QES} = 0$. Hence *ultimately* the curve *coincides* with the equator. This agrees with the last determination,—that the curve does not *cross* the equator, but coalesces with it at the infinite value of θ.

$(g_{,})$. To find the ultimate inclination to the radius-vector. We have by NAPIER,

$$\cos\phi = \tan a \cot \mathrm{ESQ}, \quad \text{or}$$
$$\tan \mathrm{ESQ} = \tan a \sec\phi.$$

But when $\phi = 0$, $\sec\phi = 1$, and $\tan \mathrm{ESQ} = \tan a$, or

$$\mathrm{ESQ} = a, \text{ or } \mathrm{ESQ} = \pi + a, \text{ as we should expect.}$$

3. We shall next take (6′), namely $\tan(-\phi)\,\epsilon^{\cot a \cdot \theta}$.

$(a_{,,})$. Let $\phi = \pi$; then $\tan(-\phi) = 0$, or $\log\tan\phi = \dfrac{1}{0}$; and hence since $\epsilon \angle 1$, all its powers are greater than 1, and consequently $\theta = -\dfrac{1}{0}$. The curve, therefore, winds round P′ as a polar asymptote.

$(b_{,,})$. Let $\phi = \dfrac{\pi}{2}$. Then $\log\tan(-\phi) = +\dfrac{1}{0} =$ infinity; or $\theta = +\dfrac{1}{0}$. That is, the curve approximates continually towards the equator, and only meets it when θ has obtained an infinite positive value.

($c_{,,}$). At all intermediate positions, the values of θ will be the same as in the other branch ($c_{,}$) we had for the supplement of these values. Thus if $\phi_{,}$ be the value of ϕ at any point of ($c_{,}$), and $(\pi - \phi_{,})$ be the value of it in the present case; then we shall have the same value of θ adapted to both.

The cases ($d_{,,}$ $e_{,,}$ $f_{,,}$ $g_{,,}$) corresponding to (d, $e_{,}$ $f_{,}$ $g_{,}$) respectively, are now too obvious to need further remark.

4. We may now consider its projections.

($a_{,,,}$). In the *gnomonic*, we have (putting $a =$ rad),

$$r = a \tan \phi;$$

$$\therefore r = a\kappa^{\prime},$$

which is the *logarithmic spiral.*

($b_{,,,}$) On the *equatorial cylinder*, referring it to the equator, ϕ is changed into its complement in the expression, and we have

$$\cot \phi' = \kappa^{\prime}, \quad \text{or} \quad \tan \phi' = \kappa^{-\prime}$$

which, when the cylinder is unrolled upon one of its tangent planes, becomes the *common or plane logarithmic curve.* This logarithmic lies on the contrary side of the meridian (which we took as origin of co-ordinates) from that upon which the logarithmic spiral lay—as it should do.

($c_{,,,}$) The *gnomonic projection of this spherical logarithmic, then, is identical* (except as to its position) *with the stereographic projection of the loxodrome.* This relation is very beautiful, and leads us, moreover, to remark a considerable number of curious relations between the two curves.

($d_{,,,}$) This relation may perhaps be more elegantly expressed thus:

From the pole of the logarithmic as a centre, describe another sphere equal to that upon which the logarithmic is traced; then the *gnomonic* projection of the logarithmic upon the second sphere is a loxodrome whose rhumb is measured by the subtangent of the logarithmic; and conversely,

From the pole of the loxodrome as a centre, describe a second sphere, equal to that upon which the loxodrome is traced; then the *stereographic* projection of the loxodrome upon the second sphere will be a logarithmic whose subtangent measures the rhumb of the loxodrome.

($e_{,,,}$) The preceding deductions apply immediately to the first branch lying on the hemisphere next to P; but similar ones also apply to that lying upon the other hemisphere: and indeed the unrolled logarithmic furnished by these two spherical symmetrical branches, correspond to the two symmetrical branches which we *ought to have* in the common logarithmic.

($f_{,,,}$) The other projections, though giving neat results in an algebraical and geometrical point of view, do not call for any special discussion. We shall therefore merely put them down for the first branch (P).

Orthographic:—

$$r = \pm \frac{ak^t}{\sqrt{1+k^{2t}}}$$

Stereographic:

$$r = -a\,(k^{-t} \mp \sqrt{1+k^{-2t}}$$

or $$r = \frac{k^t}{1 \pm \sqrt{1+k^{2t}}}$$

according as the first or second poles are taken for the second or first branch of the curve.

P. 347, l. 14, *for* $n \cos a$ *read* $x \cos a$

— 349, Eq. 6. *for* $\cos =$ *read* $\cos \phi =$

— 350, Number the figure 28.

In eq. (13.) *for* $\cot a - \theta - 1$ *read* $\cot \overline{a-\theta} - 1$

— 351, Eq. (15.) *for* $\cot \lambda \sin^2 \phi$ *read* $\cot \lambda \sin \phi$

— 352, Number the figure (29.)

— 355, In Note (B) of the former part of this dissertation, I employed the term "region" to designate the right octants of the sphere, formed by two meridians at right angles to one another and to the equator. In some subsequent inquiries, where I had occasion to frequently refer to these regions, it occurred to me that the trouble of continually writing them might be very much lessened, at the same time that greater perspicuity would be produced, by merely putting down the number itself which expressed the quadrant of either ϕ or θ in which the point spoken of was situated, this number being so marked as to prevent its being mistaken for any thing else than a statement of that circumstance. The method em-

ployed by VANDERMONDE for designating the squares of the chess-board * appeared to me admirably fitted also for this purpose, and therefore I have adopted it.

The region of revolution and the region of polar distance are conjointly written thus $\binom{m}{n}$: where n is the revolution, and m the polar distance. It signifies that the point is in the $\binom{m}{n}$th region. Thus, $\binom{3}{4}$ is the fourth quadrant of the equator, and the third of the moveable meridian. Either or both of these may be negative, as $\binom{-3}{4}$, or $\binom{-3}{-4}$, or $\binom{3}{-4}$, according as the quadrants designated by these are reckoned on the positive or negative side of the origin.

When the point is in one of the rectangular meridians, the revolution has proceeded through a certain number, p, of quadrants. This will be properly designated by $\binom{m}{\frac{p}{2}\cdot\pi}$; and when the polar distance has passed through r quadrants, we have this designated by $\binom{n}{\frac{r}{2}\cdot\pi}$. When it is at the end of p quadrants of longitude, and r of polar distance, we must then designate it $\binom{\frac{r}{2}\cdot\pi}{\frac{p}{2}\cdot\pi}$.

Lastly, if we know separately the quadrant of revolution n, or of polar distance m, but have not ascertained, or do not wish to express the remaining one, we can write it $\binom{-}{n}$ or $\binom{m}{-}$ as the case may be.

These will be fully adequate to express all possible varieties of combination of ϕ and θ.

Whether the reference to the equator in case of the equatoreal, latitudinal, or longitudinal † co-ordinates, would not be better made by the *upper*

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* Mém. de l'Acad. des Scien. 1771, pp. 566–73. The method was also applied by him to space of three dimensions.

† See the Mathematical Repository (No. 25.), for a discussion of these classes of quantities and their properties.

number, is a question that might arise in a future stage of the investigation of these subjects; but I have found no case in which it appears to be of any consequence whether we commence our expression by the distance from the pole or from the equator. If it can operate at all, it will arise from the combination of those indices in framing classifications symmetrical in their forms —an object that I can readily admit is likely to be of use in our description of loci, though I have not met with any instance where I could apply it with particular advantage. In case, however, of such method at any future time becoming desirable, we can distinguish the two methods, by prefixing P and E to the indices respectively. Thus, E $\binom{3}{2}$ designates in the second quadrant of positive revolution, and in the third positive quadrant of latitude from the said point of revolution; and P $\binom{-1}{3}$ signifies the same thing as formerly expressed by $\binom{-1}{3}$. In our investigations, however, we have always reckoned from the pole, and, at the same time, employed the indices without the P prefixed.

P. 356, line after eq. 5. *for* points upon *read* two points for each value of sin ϕ

— 357, The statement here made is erroneous—it originated in a mistake in the reductions. The equation of the locus of the intersection of the perpendicular upon the tangent with the tangent, may be easily investigated, and is at all events, though not a circle, yet a conic section. The same conclusion is obtained by Professor Gudermann *, though his processes have not the slightest resemblance to mine.

Though the analogy is not invariable as to the *identical* figures, there is still always a great resemblance. The figures, if not identical, are yet of the same family. For the sake of illustration, I shall here, instead of that, add the analyses of one or two other propositions. I must, however, first remark, that the double sign should have been prefixed to the general focal equation of the conic section at p. 358, viz.

* Crelle's Journal für der reine und angewandte Mathematik, 8er. band, 324 Seite.

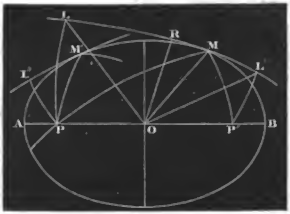
$$\tan\phi = \pm \frac{\cos 2a - \cos 2\epsilon}{\sin 2a \mp \sin 2\epsilon \cos\theta}$$

the upper denoting the ellipses and the lower the hyperbola. Also the parabola, being either an ellipse or hyperbola, should be written

$$\tan\phi = \mp \frac{\cos 2\epsilon}{1 \mp \cos\theta}$$

The focal radii-vectores make equal angles with the tangent.

Let us take P for pole, and the other parts as in the figure,



Then, per table,

$$\cos^2 \text{LMP} = \frac{\dfrac{d\phi^2}{d\theta^2}}{\dfrac{d\phi^2}{d\theta^2} + \sin^2\phi} \quad \text{.........(1.)}$$

$$= \frac{\sin^2\phi \left\{ \dfrac{\cos 2\epsilon - \cos 2(a \mp \phi)}{\cos 2a - \cos 2\epsilon} \right\}}{\sin^2\phi \left\{ 1 + \dfrac{\cos 2\epsilon - \cos 2(a \mp \phi)}{\cos 2a - \cos 2\epsilon} \right\}} \quad \text{(by XLII. 6.)}$$

$$= \frac{\cos 2\epsilon - \cos 2(a \mp \phi)}{\cos 2a - \cos 2(a \mp \phi)} \quad \text{.........(2.)}$$

Also, $$\sin^2 \text{LMP} = 1 - \cos^2 \text{LMP} = \frac{\cos 2a - \cos 2\epsilon}{\cos 2a - \cos 2(a \mp \phi)}$$

$$= \mp \frac{\cos 2a - \cos 2\epsilon}{2 \sin(2a \mp \phi) \sin\phi} \quad \text{.........(3.)}$$

Now, this being symmetrical in respect to $2a \mp \phi$ and ϕ, shews that the same angle is formed whether the radius-vector be ϕ or $2a \pm \phi$. That is, at M or M′. Now, referred to the other focus, P′M ($= 2a \mp \phi$) becomes ϕ; or, in other words, $P'M = PM'$, and $\therefore$ $P'ML = PM'L' = PML$.

Again, put $\cos 2a - \cos 2\epsilon = 2\cos^2 a - 2\cos^2 \epsilon$, then (3.) becomes

$$\sin^2 LMP = \mp \frac{\cos^2 a - \cos^2 \epsilon}{\sin(2a \mp \phi)\sin\phi} \dots\dots (4.$$

which is analogous to a well known property of plane conic sections.

Moreover,

$$\sin PL = \sin PM \sin PML$$
$$\sin P'L' = \sin P'M \sin P'ML'$$

which, multiplied, give

$$\begin{aligned} \sin PL \sin P'L' &= \sin PM \sin P'M \sin^2 PML \\ &= \mp \sin\phi \sin(2a \mp \phi) \frac{\cos^2 a - \cos^2 \epsilon}{\sin(2a \mp \phi)\sin\phi} \\ &= \mp \{\cos^2 a - \cos^2 \epsilon\} \dots\dots (5.) \\ &= \pm (\sin^2 a - \sin^2 \epsilon) \dots\dots (6.) \end{aligned}$$

In plane conic sections it is $PL \,.\, P'L' = a^2(1 - e^2) = (\text{conj. axis})^2$.

It would be easy, obviously, to pursue these researches to any extent, having so many properties of the plane conic sections before us to suggest the correlative spherical ones: this I conceive is unnecessary in the present paper. It will, however, be done in the Mathematical Repository.

P. 361, l. 7, *dele* also

NOTE E, p. 361.

I may here notice, that the error of Mr HOWARD, in assigning the nature of the curve referred to in this note, was pointed out by Mr LOWRY in the old series of the Mathematical Repository, vol. i. p. 419. The same gentleman also in that (now extremely scarce) work gave a considerable number of new and very curious theorems in spherical geometry and spherical trigonometry, as well as in the successive volumes of the new series of the Repository. He appears, indeed, to be almost the only English geometer who has given much attention to these subjects, or made any additions of consequence to our knowledge on the subject.

I have no wish, however, to disparage the work of Mr HOWARD, for it manifests much ingenuity in several parts, though it bears all the marks of its hasty composition; and that ill-fated mathematician has also the additional merit of directing Mr LOWRY's attention to that class of inquiries. But setting aside the general merit of his work, the beauty of several of his propositions, and the mathematical difficulties which were inherent in the method of investigation he employed, it may be predicted, that HOWARD's name will be remembered as long as spherical geometry is cultivated, for that beautiful theorem which he discovered,—that *the triangles upon equal bases and between equal parallel circles, being equal.* It is, indeed, one of the most beautiful properties yet known in spherical geometry, and one, perhaps, yielding to no other in the richness of its consequences. HOWARD, too, was the first systematic writer on the subject amongst us.

I had in that note mentioned all the writings that had then come to my knowledge on the subject of spherical co-ordinates. I have now to add, that Professor GUDERMANN of Cleve published a little work on the same subject in 1830, under the title of *Grundriss der Sphärik Analytischen.* This work, though I have so long delayed the completion of my own paper, with a hope to avail myself of the advantage of comparison with the work of an independent inquirer, I have never been able to procure; and, indeed, since I have tried every method that offered the least prospect of success, without avail, I scarcely now can expect to obtain it.

The learned German Professor has also published some articles on the same and collateral subjects in CRELLE's *Berlin Journal für die reine und angwandte Mathematik.* My attention was first called to this circumstance by a learned friend, who pointed out a notice in the Bulletin des Sciences Mathematiques de FERUSSAC, in which a very brief notice of one of GUDERMANN's papers was inserted. Upon referring to CRELLE for the original paper, I found it impossible to ascertain with certainty the nature of GUDERMANN's processes, which rested upon theorems and methods laid down in his treatise, and which were only referred to in the said paper. All his subsequent writings that have reference to loci, are similarly incapable of in-

terpretation without actual reference to his book. His plan appears to be, however, so far as I can infer it from certain passages in Crelle, to take the equation of a plane curve, and convert it into spherical form by writing sin or tan before the variables, according to some law determined in his work, but which cannot be distinctly gathered from his papers.

If this be not his plan, his papers are perfectly unintelligible to me. If it be his plan, it has not one feature in common with my own, except the simple fundamental idea of employing *two* superficial co-ordinates, instead of *three* linear ones, in space. I will say more. If his method is to transform the plane equations into spherical ones, by the insertion of the trigonometrical functional characteristics amongst the terms of the equation, it will certainly lead him into error. Such a principle can only be founded on projective considerations: but I have ascertained that projective considerations are insufficient even to obtain with certainty the plane from the spherical property; and much more must they be insufficient to enable us to assign the spherical from a knowledge of the plane equation.

I should be doing great injustice to Professor Gudermann were I not to speak in the highest terms of praise of the elegant theorems which he has enunciated, and to which he has affixed his own methods of proof. I have gone through the greater part of them by means of the system developed in these papers, and find them true. Such a considerable number of properties of spherical figures, whilst they are calculated to create a high opinion of the author's taste and skill, are also well adapted to interest the attention of mathematicians by their beauty, both of algebraical form and geometrical enunciation.

Erratum, Page 388, line 11. from bottom, *for* p. 35, *read* p. 293.

PLATE X.

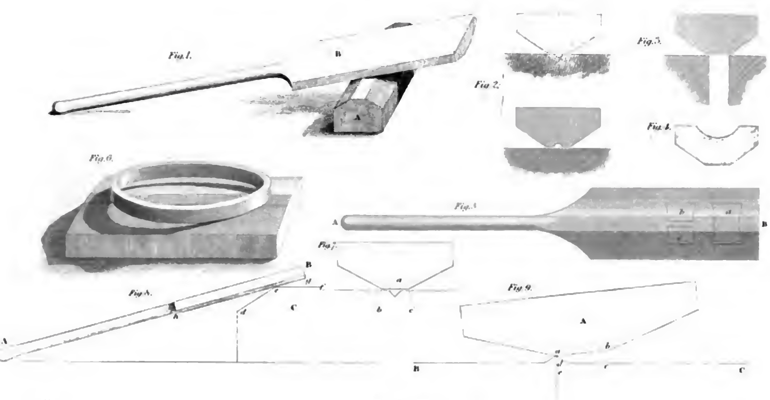
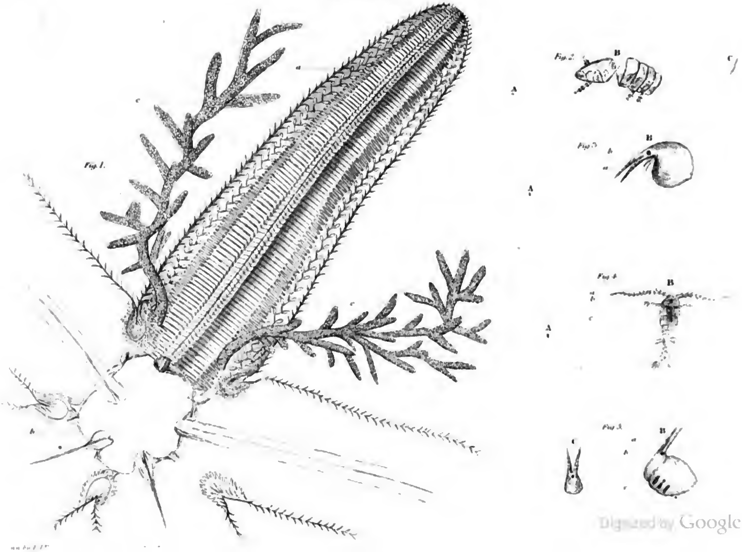


PLATE XI.



Experimental Researches regarding certain Vibrations which take place between Metallic Masses having different Temperatures. By JAMES D. FORBES, Esq. F.R.SS. L. & E., Professor of Natural Philosophy in the University of Edinburgh.

(*Read 18th March and 1st April* 1833.)

1. ON the 17th January 1831, Mr ARTHUR TREVELYAN communicated to the Royal Society of Edinburgh a paper, entitled "Notice regarding some phenomena observed during the Cooling of certain Metals placed in contact with Lead." This was the first account published of the remarkable discovery made by that gentleman, of a most curious class of phenomena, which till then was unknown to the scientific world. This paper was afterwards published, with some additions, in the 12th volume of the Transactions of that body, under the title of "Notice regarding some Observations on the Vibrations of Heated Metals."

2. Mr TREVELYAN had, in February 1829, first observed the phenomena just alluded to, which consist in certain tremulous motions accompanied by sounds, often highly musical, excited in many metals while hot, placed in contact with lead or tin, at a lower temperature. The method of rendering these conspicuous will be understood from Plate X. Fig. 1, where A represents a block of lead, and B a bar of some other metal, such as brass or copper, which is made of such a form as to vibrate readily upon two points of support, formed by the solid angles of a ridge

left on its lower surface, which is bevelled away on either side. The narrower this ridge, of course the more easily the equilibrium is disturbed. No sooner is the bar of copper, iron, or other hard metal, placed upon the lead-block, the former being heated to a moderate temperature, than visible vibrations commence, the bar oscillating upon its horizontal axis. Musical notes are not always produced, but generally under the circumstances shortly to be noticed. Soon after Mr TREVELYAN'S communication to the Royal Society of Edinburgh, the subject was taken up by Professor LESLIE and Mr FARADAY, both of whom explained the vibrations upon recognised principles, and did not conceive that any new mode of action was concerned in their production. Doubts which I ventured to entertain as to the conclusions of these eminent individuals, led me to investigate the subject experimentally, by which these doubts were strongly confirmed. Facts increased in number, and I was forced to abandon several successive hypotheses. The real difficulty of the subject, and the singular conclusions deducible from several of my experiments, led me to delay putting together the facts which I had accumulated, into the form of a paper. Nearly two years having now elapsed since the commencement of my experiments (which were almost all made in the summer of 1831), and no one else having taken up the investigation, I have resolved to publish the conclusions at which I have arrived, though such as are purely theoretical I offer with all the diffidence which a speculation connected with some of the most unexplained processes in natural science requires.

3. I propose to divide this paper into three sections,—first, on the Phenomena of Sound, as those which earliest presented themselves, and the consideration of which will pave the way to farther inquiries; second, on the Phenomena of Vibration; and, third, on the Theory of these Phenomena.

I. PHENOMENA OF SOUND.

4. Musical sounds do not always accompany the vibrations above described. There is one condition or modification of the apparatus which generally ensures their production. If a groove be made either in the bar or block, as shewn in the sections Fig. 2, in the direction of the axis of the bar, and separating the points of contact with the block, upon which the bar oscillates, we shall rarely fail in producing the sound. These sounds generally commence with a deep base note, which rises as the experiment proceeds, and as the equilibrium of temperature of the two metals approaches; sometimes rising suddenly an octave in the most fitful manner, and occasionally redescending. Mr TREVELYAN, in his paper just alluded to, has treated of the sounds thus produced, and seems to consider the phenomena introduced by the condition of the groove, of an essentially different class from the others. He assumes that the only effect of the groove is, that it may allow a current of heated air to pass through it; yet he admits that this current is not sufficient *alone* to produce musical notes, because no such occur when vibrations do not take place; nor yet, according to him, do the vibrations suffice, since they require the supposed current of air introduced by the groove to complete the effect. In his enumeration of the sources of musical notes, Mr TREVELYAN has omitted to state that the mere accumulation of impulses of any kind up to a certain number in a second, produces alone a musical sound depending upon that number. This Mr FARADAY readily shewed to be the true cause of the musical sounds produced in this experiment, namely, the number of contacts of the hot with the cold metal in a second, and he illustrated the fact by putting a cold bar of metal in vibration by means of

the corresponding expansions and contractions of a pair of sugar-tongs connected with it, when sounds quite analogous to those just alluded to were produced *. So far Mr FARADAY completely established the dependence of these sounds upon the vibrations. He did not, however, include in his explanation the action of the groove in producing them, which therefore became an object of my attention. To shew that there was no action such as the impulsion of air through an orifice, which seemed to have been contemplated by Mr TREVELYAN, I caused a hot bar to vibrate upon two pieces of lead, the two striking parts or solid angles of the bar impinging upon *different* masses of lead, at a distance of about a quarter of an inch, Fig. 3; the effect was precisely similar to that of a groove cut of the same breadth in a single piece of lead, and of the smallest possible depth. In order to shew that the motion of air had nothing whatever to do even with the tone, I allowed the instrument to acquire a steady note with the two masses of lead just described, and then carefully closed the edges of the space between the masses with adhesive paste, yet not the slightest change of note was perceptible: this was several times repeated with the same results. We must, therefore, in its fullest extent, deny the influence of any imaginary current of air in the production of the sound.

5. What, then, is the influence of the groove? The answer is simple, and easily proved by experiment, merely that the rapidity of the vibrations is in some way increased by its presence. When both surfaces are smooth, the vibrations are comparatively slow, and might almost be counted; sometimes with the form of bar I have used, they do not exceed twenty in a second. In fact, the phenomena essentially depend upon the form of apparatus em-

* Lecture at the Royal Institution April 1831, and Royal Institution Journal, No. IV, N. S.

ployed. Mr TREVELYAN practically found the influence of throwing the mass of matter to the sides of the bar, so as to render its equilibrium more unstable, and thus lengthening the period of its vibrations; and Mr FARADAY well observed, that the pressure of the finger upon the upper part of the bar, while in a state of vibration, by shortening the period of oscillation, raised the note to a great extent, and rendered the sounds musical, when before they were not so. In the bars which I have generally employed, the back was hollowed out, as shewn in the section Fig. 4, which served both to diminish the quantity of matter near the axis, and to contain a portion of mercury, the bright surface of which I found a convenient test of the existence of oscillations too minute to be readily perceived by the eye, as was generally the case when the sounds were musical. They could always be detected likewise by approaching the point of the finger gently to the bar. This will even detect vibrations which produce no visible tremor in a clear globule of mercury.

6. I trust I have now shewn that all the phenomena of sound are ultimately resolvable into those of simple vibration. There are one or two facts connected with, and indicated by, the musical tone, which we shall more particularly consider in treating of the modification of the vibrations. These are especially the sudden changes in the note of the instrument, which generally *rises* as the experiment proceeds, and the influence of the groove in raising the note, that is, rendering the vibrations more frequent.

7. We have only a remark to make upon the *absolute* velocity of these oscillations, which the sound produced affords the only accurate way of estimating. With one of the ordinary bars such as I have been in the habit of using, the highest note which I have distinctly observed and compared, was A above the middle C of the pianoforte, which corresponds to 430 vibrations in a second. From this velocity (and it is often very much

greater), the tone descends through all the lower notes down to the smallest number of vibrations producing a musical note, even to about 20 in a second.

8. From what has been now said, we are prepared to maintain that the phenomena of sound are all referable to phenomena of vibration, and we must seek an explanation of the modifications observable in the former, in those of the latter.

II. PHENOMENA OF VIBRATION.

9. When a rocking bar, such as has been above described, is put in motion upon a block of metal, the temperatures being equal, the vibrations gradually diminish in extent, and by the simple action of gravity are very speedily annihilated. That this may not be the case, as we find it is not under some circumstances already alluded to, namely, where certain different metals, and at different temperatures, are employed, we must admit the existence of some impulse which prolongs the time during which the oscillations are kept up. This impulse can only be received during the successive contacts at the two bearing points of heterogeneous metals, and may safely be assumed to depend in some way or other upon the propagation of heat, since the effect does not take place, unless the temperatures be different, nor is it indifferent which of the two kinds of metal has the highest temperature. The impulse, of whatever kind it be, resembles that derived by a pendulum from the pallets of a timekeeper, which in fact is the *sustaining power* of the mechanism.

10. The arcs of vibration of course depend, other things being equal, upon the intensity of the impulse communicated to the bar.

11. We have already noticed, that various circumstances tend to modify materially the character of the vibrations, parti-

cularly as to their distinctness and duration ; a little practice is required to distinguish between the mere mechanical oscillations of a bar once made to vibrate, and a true vibration depending on an impulse received at each successive contact. There is, in general, this difference, even where the sustaining power is very feeble, that whereas the oscillations arising merely from gravity rapidly diminish from the very first, if there be a true sustaining power, they will rather increase in energy and distinctness for some time, from the accumulated effect of successive though small impulses.

12. We proceed to examine the influence of contingent circumstances upon the intensity of these vibrations.

1. *Relation to the Specific Characters of the Substances employed.*

13. The first general law of these vibrations may be considered to be, that *they never take place between substances of the same nature.* This is probably quite general. An exception is noticed by Mr TREVELYAN, who thinks that he observed a vibration of a copper bar upon a copper block ; I am inclined to think that he had mistaken the oscillations connected with the simple law of gravity for a true vibration, as I have in vain endeavoured to repeat the experiment.

14. The second general law is, that *both substances must be metallic.* I have never seen a single exception to this law * ; Mr TREVELYAN, however, thinks that he observed a vibration upon glass. It is a remarkable fact, substantiated by experiments which will presently be mentioned, that *all* metals do not possess the property alluded to. It was natural to divide the metals into two classes, one of which might form the heated bar, the other,

* I need hardly state it as an exception that I have used *galena* (the sulphuret of lead) as a block, with success, instead of metallic lead.

the cold block; and it was also natural to suppose that metals excluded from the one class belonged to the other. I have, however, discovered, that at least two metals are perfectly inert, in either situation, namely antimony and bismuth.

15. At an early period I found (as Mr TREVELYAN also did independently), that the position of the bar and block were convertible; I mean, that the metal commonly used for the block might be employed for the bar, and *vice versa*, provided always, that the same metal was always hotter or colder (as experience showed to be necessary) than the other. Thus, Mr TREVELYAN announced that a bar of *hot* iron or copper placed on a block of *cold* lead or tin, produced vibrations; but we shall still have the same phenomena, provided we use a *cold bar* of lead or tin placed upon a *hot block* of iron or copper.

16. Mr TREVELYAN having observed that lead and tin were the metals which required to be *cold*, and that metals which he designates as "hard," such as iron and copper, must be *hot*, he naturally draws a division between two classes of metals quite distinct, each of which require certain conditions to produce the vibrations. Mr FARADAY having taken up the subject, found that hot silver vibrated on cold iron, a fact observed by silversmiths, thus forming a link between the classes, and shewing that a metal which requires to be *cold* relatively to a second metal, must be *hot* relatively to a third. Some theoretical views which we shall presently advert to, and to which experience did not seem to be opposed, led Mr FARADAY to the conclusion that the arrangement of the metals with regard to their power of vibrating with one another, was *directly* as their conducting power for heat, and *inversely* as their expansibility. The metal standing highest on the scale of metals thus formed, being necessarily the *hot* one relatively to the other, which stood lower on the same scale. These observations of Mr FARADAY, were given in

a lecture at the Royal Institution in April 1831, and were published in the Journal edited there.

17. Mr Faraday having pointed out the arrangement of metals alluded to as a theoretical result, though confirmed in some points by experiment, I conceived that the only true way of arriving at an explanation of the phenomena, would be to classify the metals by experiment in the order of their intensity and distinctness of vibrating power. In this inquiry I found many difficulties, chiefly arising from the apparently capricious nature of the effects produced, which for a long time seemed almost to baffle an attempt at classification; and it was only by reiterated series of experiments, at different times, and made in different ways, that I could satisfy myself of the degree of accuracy to which my results were entitled.

18. I soon found that the conditions of vibration depended simply on the distance between the places of the two metals employed for the bar or block, in a certain scale required to be determined. The remarkable case of iron already observed by Mr Faraday made me very desirous to extend such a law; here we have a metal which must be placed towards the middle of such a scale, since a metal above it in the scale vibrates upon it when hotter than the iron, whilst iron itself vibrates upon cold lead, which, therefore, must be placed lower in the scale; and as the intensity of vibration may be expected to be proportional to the interval in the scale, so we actually find that silver vibrates on lead much more actively and steadily than iron does.

19. I first prepared bars similar to those which have been already described, of copper, zinc, brass, iron, tin, lead, antimony, and bismuth. My earliest experiments demonstrated the small number of pairs of metals between which vibrations took place. The superiority of lead to all other metals, as the cold substance, was manifest; and in order to establish, in relation to it, the intensity of vibration of the different heated metals, it was neces-

sary to obtain some ready means of employing them all at a fixed temperature. Without entering minutely into the actual difference of temperature between the two metals requisite for producing the effect, it was sufficient to discover that the heat of boiling water answered every practical purpose; it was therefore resorted to. The temperature of the lead being 65°, we conclude that a difference of 150° between the metals is sufficient to produce the effect in the most decided manner.

Block at 65°.	Bars at 212°.
LEAD.	Zinc; vibrates briskly and steadily.
	Brass; nearly the same as zinc, but not quite so steady.
	Iron; decidedly less than brass.
	Tin; does not vibrate so well as iron, but the difference is inconsiderable.
	Antimony; not at all.
	Bismuth; not at all.

20. In pursuing these experiments, I varied them in every way I could devise, but almost always got precisely the same arrangement of vibrators. Employing a lead *bar*, I used also blocks of the different metals heated to 212°. With silver, gold, and platinum, I found it difficult to procure considerable masses of sufficient purity; and when small ones taken out of boiling water were employed and placed in a vice, I found that the loss of heat they experienced was so rapid as to destroy the comparability of the experiment. The plan I adopted for procuring an approximation to a uniform temperature, and which from reiterated trials I found susceptible of great accuracy, was the following: The piece of metal under experiment being firmly held in a vice, a drop of water was placed upon it, and a spirit-lamp applied below, so as to heat at once the metal and the vice, until the water was rapidly dissipated in the act of ebullition, at which instant the cold bar of lead was placed upon it, and the vibrations encouraged by a gentle oscillation. In this manner I

went over not only the metals which could not be conveniently tried in the other way, but also those of which I already had bars.

21. Pursuing the relations of the metals to COLD LEAD, I found, in the first place, that the position of platinum is not very different from that of tin. The mass used weighed about 7 oz., and was kindly lent to me by Dr HOPE; its form prevented its being used as a bar, and its small thickness and angular corners did not fit it for retaining a high temperature, or performing well the part of a block. When held in a vice and heated by a spirit-lamp, the vibrations of a cold lead bar were very active. From some experiments made with iron, tin, and platinum, at a temperature of 350°, I conceived that they stood as vibrators in the order just named: I had not then, however, fallen upon a method of operating upon small masses with accuracy; and subsequent experiments, often repeated, with a small thick mass lent me by Professor JAMESON, and heated till water boiled on the surface (as above explained) led me to the conclusion, that, in the other mode of experimenting, platinum had been placed too low, as might have been anticipated, and that it is at least equal to iron in vibrating power. It appears at the same time that there is little difference between platinum, iron and tin.

22. From numerous experiments with *antimony* and *bismuth*, these metals when heated appear to have no vibratory action with cold lead. This experiment was tried at a great range of temperatures, and, notwithstanding the low melting point of bismuth, I raised it by a particular arrangement to a temperature of 350° without obtaining any vibration.

23. The next experiments were made with *silver*. Even with very unfavourable apparatus it appeared remarkably active as a vibrator. A small block of heated silver placed in a hot vice, gave indications of being at the very top of the series of

vibrators, a cold lead bar being used; a result fully confirmed by subsequent experiments, in which the temperature of 212° was carefully ensured.

24. My first observations upon *gold* led me to the conclusion, that it is much inferior to silver in the scale of vibrators. Subsequent experiments led me to place it third in the scale. Standard gold was employed.

25. *Zinc* vibrates with great facility and certainty upon lead, when it has a temperature of 212°; when hotter, it is subject to some irregularities. Zinc and *brass*, to which it is closely allied, seem to stand next to gold, being very superior to platinum and iron, but considerably inferior to silver.

26. *Iron* is very nearly allied to platinum, but, from very careful experiments, is, we have seen, to be placed somewhat lower. *Tin* stands decidedly below iron, and stands in the scale next to lead, the *cold* metal employed, yet still its vibrations are very sensible.

27. From accidental circumstances, *copper* was one of the last metals I tried; when I had not the means of experimenting accurately upon silver, I had no hesitation in placing copper at the very top of the scale, so steady, forcible, and sustained were its vibrations. I finally placed it below silver, but the difference is not great. Besides the direct mode of observing the intensity of vibration on lead, I had an indirect way of confirming the results which will immediately be noticed. The arrangement of metals in relation to their intensity of vibration with lead, determined by a great series of experiments, of which I have now given the principal results, becomes the following:

Standard Silver (best.)
Copper.
Standard Gold.
Zinc.
Brass (nearly the same as Zinc.)

Platinum.
Iron.
Tin.
Antimony; does not vibrate.
Bismuth ditto.

28. This arrangement indicates very distinctly the order in which the metals possess the property or properties essential to vibration with regard to lead. But a very important inquiry is immediately suggested: Is the property of lead as the cold metal peculiar to it? or does it only require a certain space between any two metals in this scale to produce the effect? For example, lead being placed in the arrangement between tin and antimony, platinum is the third metal above it; the question is, Would platinum used as the cold metal bear the same relation to gold, the third metal above it, as lead does to platinum? This is the principle, though of course we are not bound to suppose that the energy is proportional to the number of metals interposed in the list; since we have already seen that the vibrating property in relation to cold lead is almost the same in several consecutive metals. The observation stated by Mr FARADAY, forcibly suggests the idea that a certain interval in the scale of metals is alone required to produce the effect; the lowest metal being necessarily the coolest.

29. The following are some cases of decided vibrations obtained among numerous experiments. Upon COLD TIN, heated *silver, copper, gold,* and *iron,* vibrate in the order just stated, silver being the most intense. Upon COLD IRON, *silver* vibrates decisively; my experiments, therefore, confirm the statements of Mr FARADAY. With hot *copper* I have likewise obtained distinct vibrations, though it is not possible at all times to repeat the experiment in a satisfactory manner. With hot *gold* the vibrations were dubious. Upon COLD ZINC, no hot metal except *silver* has been observed to vibrate. In this case, however, the

effect is decided. I have in no case observed that copper, silver, or gold, have any action when cold upon other metals heated.

30. It is well worthy of notice, that, from multiplied experiments with ANTIMONY and BISMUTH, and at a great range of temperature, I have been led to the conclusion, that among all the metals under trial none have any vibrating energy with antimony and bismuth, whether hotter or colder than these metals, whether used as bars or blocks. There is only a single experiment in my note-book which offers any exception. On one occasion very hot *brass* was observed to vibrate on COLD ANTIMONY; on another occasion, however, no such effect was produced. This solitary experiment appears to be one of those anomalies which have frequently exercised my patience, and retarded my progress in this delicate inquiry.

31. These experiments, it will be observed, aid us in giving a definitive arrangement to the metals in the Table before given, and which, in fact, have been resorted to in order to determine with accuracy the position of metals, of which the vibrating power with regard to lead might be somewhat doubtful. This was particularly the case with some of the metals highest on the scale: An example will illustrate the mode of operation.

32. 30th July 1831.—Experiments made with masses of silver, copper, and gold, placed in a vice, and heated with a spirit-lamp until a globule of water evaporated in violent ebullition from its surface. The cold metals were employed as bars.

HOT SILVER.

Cold Lead. Vibrates perfectly well.
...... Tin. Apparently as well as Lead.
...... Zinc. Vibrates very well.
...... Iron. Vibrates distinctly; but apparently less than Zinc.
...... Brass. Not decisively.
...... Copper. Not at all.

Hot Copper.

Cold Lead. Vibrates very well.
...... Tin. The same.
...... Zinc. Not at all; even where the temperature of the Copper is raised much above 212°.
...... Iron. Vibrates very imperfectly. It appears, however, more active than Zinc. I have formerly observed a decisive vibration of Hot Copper upon Cold Iron (11th July).
...... Brass. No Vibration.

Hot Gold.

Cold Lead. Vibrates quite readily. Nearly as with Copper.
...... Tin. Nearly as with Copper.
...... Zinc. Ditto.
...... Iron. No distinct vibration, but with considerable heat of the lamp approaches to it; it is rather more disposed to vibrate than Zinc.

By experiments analogous to these, we may see how the position of any metal is fixed by a variety of tests, which afford mutual confirmation.

33. In the course of forming the classification of metals, I was naturally led to compare it with Mr Faraday's hypothesis, that the vibration between two metals depended on the difference of their conducting powers for heat directly, and of their expansion inversely. Finding considerable deviations from this law, I was led to look for some simpler analogy.

34. The first arrangement which presented a striking similarity was that of the conducting powers of the metals for electricity. The further examination to which this remark led me, pointed out another, and perhaps not less important, analogy, which appeared not to have been before observed, namely, that when the best data are collected, the order of conducting powers of the metals for *heat* and for *electricity* is the same. I did not adopt this conclusion till after a mature examination of existing statements, and an extensive series of experiments upon the con-

ducting powers of the metals for *heat*, made with FOURIER'S Thermometer of Contact, which enabled me, where discrepancies occurred between previous observers, to ascertain the truth, and to add some new metals to the list. In the case of electricity, I was a good deal surprised to find observers more at one, than in that of heat. The result of these inquiries, which for a time withdrew my attention from the immediate subject under consideration, is contained in a paper read to the Royal Society of Edinburgh on the 7th January 1833 *.

35. The general conclusion at which I then arrived is thus stated in the paper alluded to: *That the arrangement of metallic conductors of heat does not differ more from that of those of electricity than either arrangement does alone under the hands of different observers.* I shall here quote the *provisional* arrangements which I have given in that paper, and compare them with the order of vibrations which we have recorded above.

Conductors of Heat.	Conductors of Electricity †.	Vibrators.
Gold,	Silver,	Silver,
Silver,	Copper,	Copper,
Copper,	Gold,	Gold,
Brass,	Zinc,	Zinc,
Iron,	Brass,	Brass,
Zinc,	Iron,	Platinum,
Platinum,	Platinum,	Iron,
Tin,	Tin,	Tin.
Lead,	Lead,	Lead,
Antimony,	Antimony,	Antimony,
Bismuth,	Bismuth,	Bismuth.

* The analogy to which I allude was observed by me in autumn 1831; and the experiments described in the paper just quoted were made between that period and February 1832.

† On the subject of the conducting powers for electricity, a beautiful illustration of the application of new discoveries in science to branches already known, has occurred to me since forming these lists. Mr FARADAY has shewn, that, according to

The analogy is exceedingly striking, more especially when it is stated that each arrangement has been compiled alone solely upon the evidence of experiment, and the ground upon which each metal has its place assigned to it, is fully stated in the paper alluded to for the two first columns, and in the present paper for the third. It is also worthy of remark, that the same metals seem to be most allied to one another in each of the three series. The various observers agree in treating of their respective subjects, that gold, silver, and copper are nearly allied in all: and it is probable that platinum and iron are in equally close connection*. The observations on all three points are at one in proving that there is a decided breach of continuity between lead and antimony, so marked is the change of property of the two lowest metals in the list.

36. We may now venture to enunciate a third and most important law of these phenomena; That *the vibrations take place*

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his beautiful Theory of Magnetism by rotation, the Transient Magnetic Energy (as it was formerly termed) of different Metals, should bear a relation to their conducting power of metals for electricity This is most remarkably confirmed by the following Table, given by Mr Harris in the Philosophical Transactions for 1831, which most strikingly confirms the arrangement of conductors which I have given in the text.

Transient Magnetic Energy of the Metals.

Rolled Silver,	39
.... Copper,	29
Cast	20
Rolled Gold,	16
Cast Zinc,	10
..... Tin,	6.9
..... Lead,	3.7
...... Antimony,	1.3
..... Bismuth,	0.45

* It is most probably from the great *specific heat* of iron that it stands so high in the first column.

with an intensity proportional (within certain limits) to the difference of conducting power of the two metals employed for heat (or electricity):—the metal having the least conducting power being necessarily the coldest. I have stated that the difference of conducting power must be *within certain limits,* because the anomaly of antimony and bismuth seems to be caused by this exception; and it is on the same account probably that the class of bodies which possess the vibrating property is confined to the metals; other matter being destitute of the requisite conducting power. Here antimony and bismuth almost want this characteristic property of the other metals examined. My experiments with the thermometer of contact prove their very low rank as conductors of heat, as Mr HARRIS of Plymouth, in reporting to me some experiments which he had kindly undertaken at my request, with regard to their power as electrical conductors, states in regard to bismuth, that nothing in "the form of a metal can be much worse."

2. *Influence of Figure upon the Vibrations.*

37. I have already noticed the form of the apparatus which I have generally employed. The time of the oscillations and their magnitude depend upon the figure of the vibrating mass, which seems to act just as in the case of a pendulum or rather of a rocking-stone, the impulse which it receives at each vibration appearing to be given at whatever instant of time the contact of the vibrating edges with the block is effected. This, however, must be understood within certain limits. There must be a decisive interval of time between the two contacts, for if the surface, instead of having two solid angles, as in the bar described above, merges into a continued curve, the vibrations will not take place. If, by any means, however, the period of contact of two portions of the curved surface with the block be prolonged, the impulse

will be obtained; as in the case of a silver spoon, used as a bar, where the bowl of the spoon rests upon the block. No vibrations will take place if the *handle*, which is the other point of support, terminate with a round end; should it, however, terminate with an ornamental device, which affords two points upon which it can rock, the necessary impulse will be gained; we presume, therefore, that *the time of contact of two points of the metals must be longer than that of the intermediate portions.* This condition is readily fulfilled by a vast variety of forms of apparatus, and the rudest masses of metal, such as a poker, when duly heated and placed upon lead, will produce active vibrations. The variations of tone produced upon the apparatus by mechanical interference is easily explained; if a slender rod, with metallic balls at its extremities, be placed across a vibrating bar at right angles to its axis, the time and the arcs of oscillation will be extended, the matter being thrown more to the sides; hence the note will become much lower, and vibrations previously quite insensible will become visible. Again, if while a bar is in a state of active vibration, it be gently pressed from above, the extent of its vibrations will be diminished, and the time will be reduced; hence the note will rise.

38. As it appeared essential to the experiment that the vibration should take place between two points which were longer in contact with the block than the other portions, it seemed important to determine whether the connection of these points was essential. With a view to determine this, I constructed a bar of lead of the same figure as those which I usually employed, such as AB, Fig. 5. I let into it a stud of copper *a*, of which the surface corresponded with that of the rest of the bar, and similarly two small ones *b c*, forming the two solid angles upon which the bar was to vibrate, but totally distinct from one another. Whether upon the complete bridge *a*, or upon the divided bridge *b c*, the bar, when heated, and placed upon a block of cold lead, vi-

brated precisely *as if the entire bar had been made of copper*. In an early part of this paper I described an experiment, in which the points of the block upon the bar impinged, were completely distinct pieces of metal; See Fig. 3. We therefore conclude that *the impulse is received by a distinct and separate process at each contact of the bar with the block, and that in neither case is the connection of these points in any way essential.*

39. The use of bars made entirely of the different metals is therefore quite unnecessary. A convenient form of experimental apparatus is suggested, by the following construction, which I have employed with success. A heated ring of brass or copper, three or four inches in diameter, being placed sideways upon a ridge of solid lead, with two solid angles, upon which the ring may vibrate (the plane of the ring being horizontal), the action will be extremely energetic, the impulse being given simultaneously at two points, as shewn in Fig. 6. If we had the means of firmly clamping two slips of any metal under experiment to the two points of the ring in contact with the block, by means of tightening screws, so as to substitute the material required for that of which the ring is made, we should have a convenient apparatus, requiring very small pieces of the metals to be tried, and therefore well adapted for experiments on gold, silver, &c.

40. The influence of the thickness of the metals employed, and of the extent through which the impulse may be given, early attracted my notice; and I found that thin films of metals of superior conducting power, in the form of leaf, burnished upon the lead block, did not annihilate its characteristic property. The same result in regard to simple gilding was announced by Mr TREVELYAN.

41. We have now to resume the consideration of an important point connected with figure, referred to in an early part of this paper; I mean the *groove* in the bar or block, which frequently appears essential to the production of a musical note.

We have already dismissed the supposition that it has any connection with the passage of air through that groove, and referred the effect solely to the actually observed increase of velocity in the oscillations; it still remains to explain this result. After a very careful consideration of the phenomena, I am disposed to account for it entirely upon the diminution of the surfaces in contact. It may at first sight be thought that the adhesion of two metallic surfaces must be too small to influence sensibly the time of an oscillation; when the enormous velocity of these oscillations is however considered, there can be no room for astonishment. We have shewn that there are frequently more than *five hundred* contacts and separations in a single second. The most minute adhesion must therefore clog the energy of the impulse in a way nearly insensible to our ordinary modes of impression: yet cases are not awanting where such adhesion is abundantly sensible, and especially when a metal so soft as lead is one of those employed. It is not difficult to perceive how the position of the groove or separation of pieces (for we have seen that the effect is absolutely independent of the form of the groove, provided the contact of the bar and block for a certain space be avoided) is the most favourable for producing the vibrations. The separation of surfaces may either be in the block as Fig. 7, or in the bar, as indicated by the dotted lines at *a* in the same figure: the surface of contact will thus be reduced, as there shewn, to about one half. If, instead of this, the space between the solid angles *b* and *c* had been reduced to one-half, the stability of the bar would have been materially changed, and the requisite distance between the *points d'appui* for producing an active vibration would have been deranged. By cutting out the interior space of contact, the other conditions remain unimpaired, and the adhesion is diminished to almost any required extent: in fact, the note has been most clear and steady, when the two points of contact of the block had almost the whole intermediate

space removed. The sudden changes of note before alluded to have been very satisfactorily accounted for by Mr ROBISON, as arising from a sudden movement of the bar, which, by changing its points of bearing, of course alters the velocity of vibration. The rise of tone which is usual towards the end of the experiment, depends on the diminished impulse received at each stroke, and consequent diminution of the arc of vibration.

42. Indeed the success of the whole experiment depends mainly upon the careful exclusion of adhesion between the two bodies. When merely tried under the most favourable circumstances, as when copper vibrates on lead, the experiment can hardly fail to succeed. With metals less distant from one another on the scale, more delicacy is requisite, and it is then absolutely necessary to avoid any extent of contact in regard to the length as well as breadth of the bar. The form employed originally by Mr TREVELYAN was well adapted for this effect, though the cause seemed not to be attended to: the bar AB (Fig. 8.) rested upon an obtuse angle of the lead-block C: had the bar been so inclined as to have touched the whole plane *d e*, in many cases no vibration would have taken place, and I have always been at pains to place the bar so that the angle *g e f* should be nearly equal to *h e d*. If, as has been sometimes the case, I used a block of hard metal, with an angle much more acute than that shewn at *e*, and placed a bar of lead upon it, the effect was less favourable than when the angle was more obtuse, and the contact might seem to be greater. The truth, however, was, that in this case the lead, from its softness, was *cut* by the harder metal, and a new adhesion produced, as in the action of a wedge.

43. These and many other experiments have proved to me, that, to facilitate the vibrations as much as possible, we must have a *minimum of adhesion;* thus their frequency will be increased and the note raised. Mr TREVELYAN states, that if the surfaces in contact of the two metals be highly polished, no vibration will

ensue: this manifestly depends upon the same principle, the adhesion between two perfect planes being well known to be great in amount. I have not met with so strong a case in the course of my experiments.

44. We may conclude this head by noticing, that the interference of any foreign matter between the metals (with the exception of the metallic pellicles already mentioned) seems fatal to the experiment. Dust, amalgam, a coating of oxide, or even oil-gilding, stops the vibration. The action of mercury is probably by increasing the adhesion.

3. *Influence of Temperature.*

45. We have seen that the metal of greatest conducting power must have the highest temperature in the combination of two required to produce a vibratory motion. Not merely is there no action between two metals, when the temperature of both is the same with air of an apartment, but likewise when *both* are raised to any higher temperature, for example, that of boiling water.

46. I have not ascertained what is the smallest difference of temperature requisite to produce the effect. It varies, of course, with every different pair of metals. With lead and copper, for example, the vibrations will continue much longer than with lead and tin, although in the former case the temperatures tend more rapidly to an equilibrium.

47. A difference of temperature of 150° seems to be sufficient for all practical purposes. Being anxious to investigate the properties of some metals at a definite higher temperature, I heated several bars in a cast-iron vessel full of sand, along with a thermometer, having a very long scale; this vessel was placed in another containing oil, and when the temperature had risen to 350° the bars were placed upon cold lead. On one occasion I em-

ployed copper, brass, iron, and antimony; on another iron, tin, platinum, and bismuth. I did not find, however, that the additional temperature thus gained facilitated my inquiries, and it was, in the first place, attended with considerable practical difficulties. The experiments, however, confirmed a fact which I had previously suspected, and which forms an exception to what may be considered the general law, namely, *that the intensity of vibration is proportional to the difference of temperature of the metals;* I found that at 350° iron was far more sluggish in its vibrations than at 212°. I cannot say that I remarked this in the case of copper, brass, or platinum. The fact, however, hardly admits of doubt. At an early period I had been much perplexed with some anomalies in the vibration of iron. When first taken out of a hot open fire, and just cool enough not to melt lead, its action with that metal appeared very unsatisfactory. This effect was so sensible, that I have frequently repeated with success a singularly paradoxical experiment. A bar of iron heated, suppose to 212°, being placed on a lead block, and the vibrations commenced, if a spirit-lamp was applied to the lower portion of the bar, the vibrations *are completely stopped*, and may actually be restored by immersing the lead, to which the lamp had been applied, in cold water: these singular effects I have been able to produce several times in succession during one experiment.

48. The same effects, though less striking, have been produced with zinc instead of iron, which vibrates with considerable difficulty when the temperature is raised above °212. I have been disposed to consider that every metal has its own most favourable temperature, though on what principle it is not so easy to explain.

49. It is probable that the softening of the heated metal diminishes the resiliency of the two bodies when impact takes place. I do not think that it is attributable to the softening of the lead, for I have found that iron is more disposed to vibrate

on platinum when at a moderate temperature, than when red hot. The effect may, however, be connected with the theory of the vibration.

50. Having now discussed the phenomena of sound, and of the vibrations to which we have shewn these sounds to be referrible, we shall next consider

III. THE THEORY OF THE PHENOMENA.

51. It is a curious fact how imperfectly the interest attached to the phenomena observed by Mr Trevelyan, seems to have excited enlightened curiosity. Indeed, an explanation of great simplicity, and which appeared to account for the more conspicuous phenomena, was pretty generally acquiesced in, and seems to have acted as a barrier to farther examination. It was, I believe, first thrown out by Sir John Leslie, on considering the simple facts which were brought to light by Mr Trevelyan's experiments, that they might be explained by the expansion of the cold metal at the instant of contact with the warm one, which might be supposed to give a sufficient impulse for sustaining a new vibration. Even at first sight it does appear very difficult to conceive how, when the vibrations are increased to 500 or more in a second, a process depending upon so slow an operation as the conduction of heat, should cause the metal to expand and contract successively by a finite quantity. The effect has every appearance of being one of active and almost instantaneous repulsion, and bears no resemblance whatever to the slow mechanical elevation of the surface by the process of expansion. But such inferences are often erroneous; it became, therefore, most important to inquire how far the hypothesis was applicable to various forms of the experiment, particularly to the different properties in this respect, of various substances.

52. This more difficult task was undertaken by Mr Faraday; and in a lecture on the subject which I was fortunate enough to hear at the Royal Institution in April 1831, he freed the subject (as we have already seen) from many of the difficulties with which it had been surrounded, and illustrated the theory which he supported, in that happy style for which he is so remarkable.

53. The principle which he adopted was fundamentally the same as that of Sir John Leslie, but he added an explanation of the influence of the properties of different metals upon the phenomena. According to his view, the *hot* metal should have a higher conducting power, and a smaller expansion by heat, than the *cold* one, and the arrangements of the metals as vibrators depend, according to him, upon this principle. To employ the official statement of his views contained in the Royal Institution Journal *, "the superiority of lead, as the cold metal was referred to its great expansive force by heat, combined with its deficient conducting power, which is not a fifth of that of copper, silver, or gold; so that the heat accumulates much more at the point of contact in it, than it could do in the latter metals, and produces an expansion in that respect proportionally greater."

54. I certainly approach with deference any opinion expressed by a philosopher of the reputation and acuteness of Mr Faraday, and nothing but a strong conviction, entertained chiefly upon the *general* grounds already alluded to, could have induced me to spend my time in an investigation, which he considered decided upon some of the simplest principles pf physics. My dissatisfaction with the explanation increased the more I thought of it, and the more closely I analyzed the natural pro-

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* New Series, No. IV. p. 119, 122.

cess which he had traced out. I consider it essential to point out on what grounds I dissented from a theory supported by two of the first names in British science, before I proceed to give any opinion of my own, which may perhaps be liable to equally strong objections, but the data of which are not the less valuable as physical facts.

55. Waving all minor objections, I conceive that the process of the communication of heat, and consequently its effects, would be very different from what has been stated in the passage just quoted. Let Fig. 9. represent on an exaggerated scale the presumed state of the apparatus in the middle of an oscillation; the hot bar A, whilst performing its vibration upon one of the solid angles *a*, has expanded a portion of the cold block BC into a hillock at *d*; when the semi-vibration is completed, the angle *b* of the bar will touch the block, and raise a new hillock at the corresponding point *c*, whilst the elevation at *d* subsides; and so on alternately. Let us conceive that *de* is the finite depth to which heat is communicated in the minute portion of time occupied by a semi-vibration, a depth so small as to be inappreciable by the senses, and insignificant compared to the distances between the points of impact *dc*. The elevation or height of the hillock *da* is the amount of expansion of the element *de*, by the accession of temperature received during a semi-vibration; the question is, what relation will this expansion, or acquired vantage-ground for the commencement of a new vibration, bear to the nature of the block BC, considering the nature and temperature of the bar A, and the initial temperature of the block, to be constant? It surely requires no elaborate demonstration to prove that the amount of caloric which passes into the block must increase with the conducting power of the material. Upon the very fundamental axioms of the theory of heat, the amount of caloric which passes from a molecule A into a molecule B in an infinitely short interval of time, is proportional to

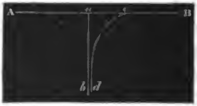
the difference of the temperatures of the molecules * combined with the conducting power of B (that of A being considered constant), and with the element of the time. Or, putting the temperature of A $= \alpha$, that of B $= \beta$, and its conducting power $=$ K, and the element of time $= dt$, the proportion of caloric transmitted will be

$$(\alpha - \beta). \text{K}. dt.$$

56. It appears to me indubitable, that whether the time be short or long, the quantity of caloric transmitted, and the consequent amount of expansion, must increase with K. The idea of an accumulation of heat at the surface producing more effect than a rapid communication with the interior, is obviously an oversight. For if the heat be accumulated at the surface, the temperature of that surface rapidly approaching to that of the source of heat A, will, in the same ratio, diminish the amount of heat received; and it can require no demonstration to prove, that the expansion depends solely upon the amount of temperature acquired above its initial temperature by the prism of metal, which by its expansion is to raise the bar from *d* to *a*, (Fig. 9.) modified of course by the amount of expansion proper to any substance employed †.

* For such small differences of temperature the Newtonian law may be viewed as absolutely accurate.

† In fact, let AB represent the surface of the body, receiving heat at the point *a*; and let *a b* be a line normal to the surface, consequently the expansion of which is to produce the elevation at the point *a*. The ordinates of the curve *cd* may represent the acquired temperatures, and the total acquired temperature will be denoted by the area of the curve, to which likewise the expansion will be proportional without regard to its particular form (the distribution of heat), which will vary with the conducting power; and although it is very possible that the ordinate *ac* of the curve may be greatest in a bad conductor, it is very easy to see that the total area never can.



Consequently this amount, or $d\, a$, will be proportional to

$$\frac{E}{c}(\alpha - \beta).\ K.\ dt$$

E being the measure of expansibility, c the capacity for heat of the substance, taken by *volume* not by *weight*.

57. Hence it appears to be quite obvious, that as far as conducting power is concerned, *both bar and block* should have it in the highest possible degree. It would be quite essential, too, upon this explanation, that the cold metal should expand more than the hot one, otherwise the loss of elevation by the contraction of the warm metal will equal or exceed the vantage-ground for the new vibration gained by the expansion of the cold one. By both these criteria Mr FARADAY'S theory seems to be deficient:—I need only point out the position of zinc, which, with greater expansibility than lead or tin, occupies so high a place in the list of vibrators, and cannot be used as the cold metal with any other except silver; according to the theory, zinc ought to vibrate far better upon zinc than upon lead or tin. Silver again vibrates upon cold iron, although its expansibility is a half greater. Such facts as these seem absolutely unaccountable upon the hypothesis of expansion.

58. The objections which I took *in limine* to the explanation of Sir JOHN LESLIE and Mr FARADAY, (which was adopted by Mr TREVELYAN in the paper printed in the Edinburgh Transactions), were strengthened, and I may say rendered decisive, by my subsequent experiments, the results of which have been detailed in a previous part of this paper. For nearly two years I have been constantly expecting to see some systematic examination of these curious facts, but the public seems to have rested satisfied with the ascription of them to a simple and acknowledged effect of heat. They have hardly been noticed in the Journals, and foreigners complain of the few data afforded by English works on the subject. An article by Professor

MÜNCKE of Heidelberg in *Poggendorff's Annalen* *, consists chiefly of the translation of a very brief notice, which I had published in the Edinburgh Journal of Science †, but contains no new observations. Having shewn the reasons which led me to dissent from the opinions at first proposed, I shall now explain the views which I have been led to entertain from the study of the phenomena.

59. I shall first recapitulate the general laws at which we have arrived.

1*st*, The vibrations never take place between substances of the same nature. Art. 13.

2*d*, Both substances must be metallic. Art. 14.

3*d*, The vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting powers of the metals for heat (or electricity): The metal having the least conducting power being necessarily the coldest. Art. 36.

4*th*, The time of contact of two points of the metals must be longer than that of the intermediate portions. Art. 37.

5*th*, The impulse is received by a distinct and separate process at each contact of the bar with the block, and in no case is the metallic connection of the bearing points in the bar, or those of the block, in any way essential. Art. 38.

6*th*, The intensity of the vibration is (under certain exceptions) proportional to the difference of temperature of the metals. Art. 47.

60. In order to satisfy these various conditions, we shall find that the range of hypotheses is not great. During my experiments I was for a long time attached to the idea of a thermo-electric action. The hypotheses which I assumed to explain the steps of it I was forced successively to abandon, and the total

* 1832, No. III., p. 466. † New Series, No. XI.

want of connexion of the order of the metals as vibrators with their thermo-electric properties (and especially the absolute inertness of antimony and bismuth), convinced me, after a long series of experiments, undertaken with this view, that I was wrong.

61. The strict and simple connexion with the conducting powers of the metals for heat and electricity, afforded a firm basis for speculation, and I was soon forced to consider heat as the sole agent in the case, all idea of electricity being necessarily abandoned, as soon as it was established that *thermo*-electricity had no share in the action. The general laws above quoted seem to be all resolvable into this, "that there is a repulsive action exercised in the transmission of heat from one body into another, which has a less power of conducting it." These repulsions only take place between bodies having a certain amount of conducting power, below which some metals fall; it must be excitable in a most minute space of time; and is energetic in proportion to the difference of conducting power of the substances, and to their difference of temperature.

62. It seems most probable, therefore, that the repulsive action alluded to, depends on the internal motions of heat itself. It were easy to frame a hypothesis which would be sufficiently plausible, and represent the phenomena. I forbear, however, from doing it at present, because our ignorance of the internal constitution of bodies, and the mechanical process of the conduction of bodies, is such as to render hypothetical reasoning upon such data almost useless. That repulsion does exist in the case of heat, can hardly admit of a doubt. The reason that we cannot render it visible in ordinary cases, is no doubt that the repulsion of the heat in two approximate molecules of bodies is too small to be weighed in our balances. Consequently, two bodies equally heated and placed together, manifest no sensible repulsion. In such a case every portion of heat is kept in equilibrium by the equal and opposite repulsions of the molecules on each side of it, which is the

case when heat is uniformly diffused through a body, and which is manifested by that universal tendency to diffusion. Hence the element of heat is in a state of equilibrium, and the only force which could be excited successfully to produce a separation, would be between the heat residing in the *last* molecule of one body, and the *first* of a separate one in contact with it, but not bound to it by cohesive attraction. Suppose, however, this second or free body cooler than the other, a current of heat will be immediately created, which, as it is more or less easily received by the cold body than parted with by the hot, will create a stagnation, or a rarefaction of the elements of heat, respectively; in the former case producing a repulsive action, or recoil through the whole string of elements set in motion; in the latter we are led to anticipate that the action would be attractive. If this view be correct, (and being theoretical, I do not attach great importance to it,) it is easy to see why repulsion takes place only when the cool body has less conducting power than the hot, and why the repulsive energy depends on the difference of these conducting powers. In the case of very bad conductors, such as antimony and bismuth, I conceive that the current has not had time to establish itself.

63. In the case of electricity, a remarkable similarity of effect is observable, depending on the conducting power of the material through which it passes. All those remarkable repulsive actions which produce destructive effects in the case of lightning, take place during the accumulation of impulses in bad conductors *.

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* I might point out another analogy in the sudden and forcible action of the hydraulic ram, where the accumulated effect of small impulses produces sudden and intense results, but I am afraid of extending unwarrantably such speculative analogies. The two preceding paragraphs of this paper have been somewhat modified since it was first read.

64. I have been led to entertain the idea of a new species of mechanical agency in heat, not from a love of introducing novel principles, but after having been driven by experiment from the hypotheses to which I was at first entirely attached. Although the mechanical effect of the repulsive power of heat cannot be said ever to have been demonstrated, experiments are not wanting which seem to be quite inexplicable without its aid, or some other principle not yet recognised in science.

65. Several ingenious French experimentalists have furnished us with facts, which, though not completely established as belonging to any peculiar class of phenomena, and therefore not generally admitted into systematic works, are not the less worthy of notice. Those which bear most directly on our present speculation were observed by M. Fresnel *; namely, the repulsion of disks of mica, of which one was placed at the extremity of a delicately suspended needle *in vacuo*, and when the disks were in contact, heated by means of a ray of solar light concentrated by a lens. M. Saigey † has also described a class of similar phenomena observed by himself, with a considerable number of metals, which, after rejecting the influence of aerial currents, of electricity, magnetism, &c., he ascribes to the repulsive action of radiant heat at sensible distances. There are several other experiments on record which seem to require a similar explanation, but I apprehend that the present are the first to establish the existence of some species of mechanical repulsion in the propagation of heat, a principle which can hardly fail to be applicable to the explanation of many natural phenomena.

Greenhill, Edinburgh,
19th February 1833.

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* *Annales de Chimie et de Physique*, xxix. 57. and 107.

† See several successive articles in the *Bulletin des Sciences Mathematiques*, tom. ix. See also Pouillet, *Elemens de Physique*.

Observations on the Natural History of the Salmon, Herring, and Vendace. By Robert Knox, F. R. S. Ed.

(Read 7th and 21st Jan. 1833.)

PART I.—THE SALMON.

The inquiries of scientific persons into the various departments of knowledge, have very generally for their sole object the investigation of truth, and the progress of science, without any reference to trade or the usual business of life. Hence to the practical man they seem generally dry and without interest, his mind being directed towards immediate utility, public or individual. Should I in the course of the present inquiry, (which in its essential nature is strictly anatomical and physiological, and can be investigated with advantage only by the anatomist and physiologist,) seem to the strictly scientific to have paid too much attention to practical details, apparently unconnected with the scientific part, and consequently out of the line of my vocation, my apology is, that in the course of the observations, I found this to be unavoidable, although my utmost efforts were made to prevent the inquiry running into this laborious and expensive train; and I entertain a firm belief, that, on a careful perusal of the observations, taking all matters into consideration, it will be found that I could not have done otherwise.

It will, I hope, be admitted, that an inquiry into the habits of gregarious fishes, necessitates more than a hasty glance at a few specimens; this mode had been often attempted, but had constantly failed in discovering the truth. The nature of the food of the Herring, Corregonus, and Salmon, was not to be stumbled on by accident. I feel happy in having to offer it as a direct result of patient scientific inquiry. The obstacles in the way of truth and of discovery in respect to these animals, has been finely glanced at in a short notice by a distinguished geologist and scientific man, to which my attention has been called since these memoirs went to the press *.

When the evidence before the Parliamentary Committee on the Salmon question was published, I examined that evidence, with a view to the obtaining additional knowledge as to the generation of osseous fishes. In this I was much disappointed: on comparing the evidence with itself, and with my own inquiries, I found it to be, in my judgment, an inextricable mass of confusion and error. Having satisfied myself as to this, I, after mature deliberation, but still with considerable reluctance, proceeded to extend my inquiries into the natural history of the Salmon, Herring, and their congenerous species; the result I have now the honour to submit to the public in the form of a brief text. The Appendix contains a mass of matter, criticisms, &c. in support of the opinions contained in the text, and will shew what all preceding observers have evidently neglected, the particular and personal nature of my inquiries,—the varied nature and extent of the field of observation,—the infinite care taken to avoid error,—and that scarcely any thing herein stated rests on hearsay evidence, or on conjecture. Of my competency to make correct anatomical and physiological remarks, no reasonable person I hope, who is at all acquainted with the nature of my pursuits, will have any

* See a Memoir by Dr MacCulloch in Brande's Journal for 1829.

doubt; but respecting this it would of course be unbecoming in me to make any further remarks. I may take the liberty, however, once for all, of cautioning those who may trouble themselves with a perusal of these memoirs, respecting criticisms, which no doubt will be offered in abundance, with a boldness not unfrequently proportioned to the ignorance of the critic *. When discoveries are made in any of the exact sciences, as of astronomy, optics, mathematics, &c. the merit or accuracy of such discoveries is investigated and criticised by astronomers, opticians, mathematicians, &c., that is, by persons competent to the task, and entitled to criticise by a parity of ability and of attainments. But in natural history it is otherwise: for although there are many problems in natural history of extremely difficult solution, requiring an extended acquaintance with anatomy and physiology, two sciences of as difficult acquisition as any I know of; yet I have met with thousands of persons, otherwise well educated, who cannot be made to understand this very simple fact; but who, on the contrary, persist in believing that any one may make observations on zoology, whatever may be their abilities or previous education. Those readers, then, who take an interest in the zoology of the salmon and herring, would perhaps do well to consider that a familiarity with fishes, as an amateur angler or fishmonger, does not constitute a zoologist; the application has been made to other sciences, and I know of no reason why zoology should be held singular in this respect.

The memoir is divided into sections, with a view chiefly to the convenience of the reader.

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* We have already seen a person assert, in open defiance of the statements of all practical fishermen, and of every writer on natural history, from Linne downwards to Professor Rennie, that the food of the Herring was known to every body!! The object of such remarks cannot be mistaken. The respect due to science, and to this Society, precludes me from noticing such statements at greater length in this place. A few additional remarks will be found in the Appendix.

SECTION I.

Naturalists are not agreed as to the number of species constituting the Salmon and Trout kind. For practical purposes the following arrangement may be admitted: the true Salmon, including the Grilse; the Salmon-Trout, merely an inferior kind of Salmon, and whose natural history I shall give in a separate memoir, and including the Whitling of the Tay; the Herling abounding in the Solway, and which some naturalists have unhappily confounded with the Salmon-Trout or Whitling *. Of

* When herling first ascend a river, and are taken shortly after their ascent, but within that part of the river influenced by the tide, they are clear, silvery, and covered with scales, compared with what they become after a short residence in fresh water above the influence of the tide. In July and August, for example, herling taken in the stake-net, of the Solway or even in the Nith, as high or a little higher than the port of Kelton, are in this prime state, and moreover have a redness of flesh, giving the fish a general vermilion colour in certain positions, and an excellence of flesh as an article of food in no shape inferior to the grilse. Their stomachs and intestines are empty, or contain only the peculiar salmon-food. They will remain several days in this part of the Nith without resorting to any other description of food, although the river at the time actually abounds with herring-fry, minnow, small shrimps (forming exclusively the food of a very superior sort of river-trout). On the other hand, fish the river Annan at about twelve miles in a direct line from its mouth, in the latter end of September, and the herling will be found in sufficient abundance, but altered, first, as to external appearance, it has assumed much of the sea-trout: its organs of generation (male or female) are rapidly advancing to the spawning condition. The stomach contains minnow and the ordinary food of trout; and yet it is worthy of notice, that when kept they do not run so rapidly into a putrescent state as the common trout taken at the same time. The females are about one-half more numerous than the males, and the quality of the flesh is little, if at all, better than that of the common trout. It was remarked, also, that all the herlings taken at this time and in this locality, were advancing rapidly into the breeding condition; whereas many common trout, equally large, were not altering into this state, being trout which evidently would have remained barren throughout this season at least. The flesh of these was superior to that of the herling caught as above.

the River Trout, of which I imagine there are at least two species frequenting our rivers and lakes, I may remark, that the first is the Yellow Trout; the second I have named the Parr-Trout. This latter is often confounded, even by practical fishers, with the Parr, Brindlin, Fingerling or Samlet, and the mistake has given rise to innumerable errors and endless disputation. In their nature, habits, and locality, as I shall afterwards shew, they are very distinct. Of this parr-trout one kind is found to be red when opened, and is a very superior article of food to the others, which are always white, however fed. The parr, though in some measure unimportant in itself, by reason of its want of bulk, has nevertheless received from us a degree of attention almost equal to that bestowed on the salmon, and which seemed in some measure necessary by its supposed connexion with the natural history of the salmon: its history, along with that of the herling, will form a subject for a separate memoir. Closely allied to the salmon is the Vendace of Lochmaben, generally esteemed by naturalists as a Corregonus. This particular species, however, is of rare locality, confined indeed to a very few lakes in Britain, of excellent flavour as an article of food, and in this respect perhaps second only to the salmon. Its food, as happens with so many gregarious fishes, was unknown; like the herring, to which it is also closely allied, it was supposed by the common people to live by suction, on air and water, &c. The more rational conjecture of naturalists, that its food is of a vegetable nature, proved an error: we first discovered and put beyond a doubt, that its usual food is the microscopic entomostraceous animals with which the lake abounds. This guided us in an unerring way to the discovery of the food of the herring. But the history of these fishes will form the subject of the Second Part of these observations. Through the fish called Corregonus, of which the Vendace of Lochmaben offers a good example, the Salmonidæ are allied to the Clupeadæ, or the Sal-

mon to the Herring. This happens in our own country; in other countries there are perhaps some other fishes which more completely supply the link between these two most important tribes. Captain FRANKLIN and his intrepid party, who twice visited the Arctic Regions of America, mentions their subsisting mostly through the winter on a fish called the Herring Salmon of the Bear Lake. I have not seen this fish nor any delineations of it, but presume it to have been a Corregonus closely allied to the salmon and herring, and constituting an intermediate link connecting these two great families.

SECTION II.

The admirable flavour of the true salmon, his profuse increase and value in the market, render his food and extent of migration worthy objects of the most careful inquiry. This was, however, surrounded with great difficulties, and had given rise to many opinions more or less probable, but all in their essence conjectural, or nearly so. As a proof of the difficulty of the inquiry, it being unnecessary to cite more here, I shall content myself with quoting a passage from a very recent work (1833,) on Natural History, "The Complete Angler of IZAAK WALTON," edited by Mr RENNIE, Professor of Zoology, King's College, London. In 1653, WALTON found nothing in the stomach of the Fordige Trout; and in a note, in the year 1833, Mr RENNIE adds, "The same is true of the salmon, which has never any thing besides a yellow fluid in his stomach when caught: the same is also true of the herring." The food of the true salmon, on which all his estimable qualities, and in my opinion, his very existence, depend, and which he can obtain only in the ocean, we have found to be the ova or eggs of various kinds of the echinodermata, and of some of the crustacea. From the richness of the food on which the true salmon solely subsists, arises, at least to

a certain extent, the excellent qualities of the fish as an article of food. Something, however, must be ascribed to a specific difference in the fish itself; for though we have ascertained that the salmon-trout lives very much in some localities on the same kind of food as the true salmon, yet under no circumstances does this fish acquire the same exquisite flavour as the true salmon: hence the real habitat of the true salmon is the shores and bays which most abound with this kind of food. When the salmon first takes to the estuary and to the river, whether beyond or within the influence of the tide, he does not feed unless the estuary should happen to contain this peculiar kind of food; food congenial to other and analogous fishes, (however closely they may be allied to him) is neglected by him *; and as in no case can he find his proper and peculiar food in fresh water, he deteriorates constantly whilst he remains. Neither the estuary, the brackish water, and much less the fresh water river constitute his habitat; he takes to them solely on account of their being comprised within the range of his migration. Many salmon ascend and descend rivers with every tide, and, like their congener the trout, rush up rivers when flooded. The regulating cause of such movements in the trout is, I am satisfied, the search for food; but every fact disproves this application to the salmon. Their farther migration up rivers beyond the influence of the tide, will be explained when we speak of the history of salmon as a smolt. To go further into this matter here would but lead to repetition. On the other hand, the salmon-trout is by no means so nice in respect to food. Besides preying on the food of the true salmon, it takes very readily the sand-eels and herring fry; and these it finds in more or less abundance in estuaries and at the mouths of rivers. Thus, both in its specific nature, and in respect to its indiscriminate

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* Experiments are wanting to determine to what extent the spawned salmon of the true salmon kind feed in rivers during the spring months; but on this, as on most of the disputed points, see the Appendix generally.

mode of feeding, it is inferior to the true salmon; and I must say, that, during my investigations, I have found this fact, I mean his inferior quality as food for man, known to most persons in the least degree practically acquainted with fishes. The want of extended observation in these matters has led some distinguished persons into very foolish theories. Sir Humphry Davy had a theory that all the salmon kind fed on one kind of food, but being unable to explain why the true salmon when taken in rivers even newly run, have the stomach quite empty, he imagined a silly notion, that this abstemiousness of the salmon arose from an instinct instructing him not to load his stomach with food on the eve of a long journey. The truth is, that whilst the stomach of the true salmon seems always empty, that of the sea or salmon trout, from being a coarser feeding fish, is generally quite full. And as to the long journey Sir Humphry thinks they are about to take, he seems not to have known that the salmon, during all the early part of the season, seem to be ascending only so far as the river is influenced by the tide, and generally returning with it when allowed to do so. A glance into a stake-net fish-house, and a very brief inspection of a *stage-net fishery* (such as those now in use on the Dee at Kirkcudbright, from the bridge below Tongueland to the lowest ebb of the tide), would have pointed out to this excellent chemist the true explanation.

The true salmon when in rivers above the highest point of the flood-tide, and as an inhabitant thereof, will be found, with certain exceptions to be afterwards explained, out of condition, and unfit for any market in the kingdom: he is in the river, if above the tideway, and after the height of summer, for the special purpose of depositing his spawn: the exceptions to this will be explained hereafter. It naturally happens that whilst in rivers, and still active, he suffers greatly for want of food, and may no doubt sometimes be taken with the ordinary bait of trout; but so far from there being any extensive series of

experiments to prove that he feeds on this kind of food, every recorded fact proves the very reverse: they become at last disagreeable objects to look at; I have opened the stomach of a fish killed by the poacher in the month of October, nearly 100 miles from the ocean, with the peculiar food and none else, still in the intestines, and this in probably the finest trouting stream in Scotland, and of course full of trout food, which, in this extended run, occupying at the least several days, he had altogether declined touching. These important facts seem to me to determine the following points: the true and only feeding ground, which is strictly the ocean; the breeding ground, which is the fresh water stream, whether principal or tributary. The part of the river influenced by the tide is a kind of debateable ground in which he neither feeds nor breeds, but which a few enter with the tide, or during great floods of the river, seemingly because it lies within their ultimate range of migration. Of the causes which may induce the true salmon to take to rivers besides the great and avowed one, the propagation of his species, there may be others difficult to understand. The facts which I have collected on this point are not many, but by being recorded, they may serve as a nucleus to others; they tend to shew that those parasitical animals which prey on him whilst in salt water, as the *Monoculus piscinus*, which adheres to the integuments, and the tape-worm * which generally fills the pancreatic cœca, are disposed to leave him, and probably do entirely quit, during his residence in fresh water: the fact is beyond dispute with regard to the first of these parasitical animals, but is unimportant when compared with the history of the tape-worm, whose continued presence, growth, and reproduction, must ultimately, unless checked by some wise provision in Nature, prove his destruction as it does in

* Mr Rudolphi arranges this kind of tape worm with the "Botriocephali." His authority in this respect I should suppose to be superior to every other.

man. In this way, I believe a residence in fresh water to be destructive to the tape-worm in the salmon, and to remove from him, or at least tend to remove from him, a cause of disease and death. But I would not have it believed that I speak in this matter from any very extended opportunities for observation; these are difficult to be got, and entail infinite labour and great expenses on the observer. And yet his sojourn in fresh water gives rise to other evils, for it is then that a new parasite, the Lernæa, fastens on his gills, organs of vital importance: to these latter, sea water seems certainly to prove a poison *. We have experimented also on the tape-worm when removed from the body of the salmon, and found it speedily died with apparent pain, whether put into fresh river or sea water.

SECTION III.

Many excellent observers have described, with more or less accuracy, the generation of the salmon, the growth and progress of the smolt, and the descent of the kelt or spawned fish to the ocean; but I know of no continued series of observations on the subject, published by any one, of an authentic nature, and so as to admit of no doubt. To remove this chasm, and to give to the naturalist a nucleus whereon to build future observation, I have thought it right to detail at considerable length the following history of the Salmon Smolt, from its first deposition under the

* The history of the tape-worm as it affects the human species, is a subject well deserving a most extended inquiry. It is stated on the authority of Mr PEARCE, who travelled and resided a considerable time in Abyssinia, that the natives of that country are exceedingly subject to tape-worm. After what I observed in Africa, where the disease, on a particular occasion, attacked almost every one, I have no hesitation in ascribing the cause of its frequency in Abyssinia to the use of unwholesome beef.—*See a Memoir in the Edinburgh Medical and Surgical Journal for* 1822.

gravel in the form of an egg, to its ultimate disappearance from the fresh water streams, which formed its habitat whilst infantile; remarking, that every thing stated therein fell under my own immediate personal observation. I have thought it preferable thus to narrate at some length, and almost in the order of their occurrence, a series of observations on the generation of the ova, to any other mode of describing the natural history of the salmon-smolt or fry. It is difficult to speak of matters of this kind in the abstract, or under the form of conclusions or results; too much has been already attempted in this way. Besides, I had it not altogether in my power to compare these observations with my previous ones, having taken at the time few notes of any consequence, and trusted too much to recollection; and as no other person had given to the public a series of similar observations, I have thought it better to detail them to this learned Body, thus connecting the natural history of the salmon fry with the grown or adult salmon.

THE SMOLT, FRY, OR YOUNG OF THE SALMON.

November 2.—Salmon are observed to be spawning in the various tributary streams of the Tweed which join that river from the north, and a pair are watched. The ova observed to be deposited near the sources of the stream on the 2d November, and covered up with gravel in the usual way.

February 25.—On the 25th February, or 116 days after being deposited, the ova, on being dug up, are found to be unchanged. If removed at this time, and preserved in bottles filled with water, the development of the egg may be hastened almost immediately, by being put into warm rooms; it is not necessary to change the water. The fry so hatched, *i. e.* artificially, cannot be preserved alive in bottles longer than ten days; they eat nothing during their confinement.

March 23.—The ova now changing; the outer shell cast; the fry are lying imbedded in the gravel, as fishes somewhat less than an inch in length, being now twenty weeks from the period of their deposition.

April 1.—On reopening the spawning bed, most of the fry had quitted it by ascending through the gravel. During a former series of observations, I have found the ova embedded in the gravel unchanged on the 10th April, and as fry or fishes, but still imbedded in the gravel, on the 17th: they were taken that year with fly, as Smolts on the 22d April, about the size of the little finger.

April 19.—Many taken, eight and even nine inches long, in excellent condition; the rivers now abound with them, and their food is exactly the same as the trout: after death they run rapidly into putrescence; they will not bear handling, and uniformly die if an attempt be made to preserve them out of their native element, the pure running streams of rivers.

May 5.—Still abound in the tributary streams, but are not so numerous as before; they are not increased in size, and are in all probability fry of a later deposit. The extreme of their growth seems to be about nine inches, at least none were taken larger than this.

The pair of salmon which were watched, were from 14 to 16 lb. weight, and were seemingly the ordinary Tweed salmon. The river (the Whitadder) has its source in a mountainous country, and at an elevation of 900 feet above the level of the sea. At this time (November) the stream abounded in all the different kinds of salmon usually taken in the Tweed, with which this stream communicates at a short distance from Berwick. They were engaged everywhere in spawning, this being the usual time in which that act is carried on. They commence in the latter end of October, and some are found to spawn so late as the middle of January, but no such occurrence has ever been observed later, nor would it be easy

to persuade the inhabitants of the banks of these rivers that salmon ever spawn after the 15th January. The ova we now speak of were deposited on the 2d November, and this was the exact period during which the salmon were observed to be spawning in these streams. From *data* with which we are acquainted, we are assured that at least 150 salmon of different kinds had passed up into the streams now spoken of. These streams may be thus described: Two streams running from high mountain ground; unite with each other after a course of about five miles each, they are small, but very constant streams, and even in the driest season, at the junction they measure about twenty feet broad, by about ten or twelve inches in depth of stream: there are some deep pools, (say ten feet) in the course of one of these rivulets. The stream so formed by the junction of these two, is joined at the distance of about three miles lower down by a third rivulet. The united stream, even in the driest season, will then measure upwards of sixty feet in breadth, with a depth of about sixteen or eighteen inches. From this point the river runs through an open country for the distance of about three miles farther, where it is joined by a stream nearly equal to itself in strength; but our observations during the spring were limited altogether to the river above the junction with this latter river.

The spawning bed made the subject of observation, was placed at the foot of a pretty long and placid pool, and just at the top of a stream where the water first begins to feel the effects of the approaching descent. The water was here about fifteen feet broad, with a depth of about six inches. The breadth of the bed seemed to be about eight feet, its length might be three or four feet, the whole having rather an oval form. The bed was easily marked out by the eye in consequence of the gravel having the appearance of washed gravel; and this arises from the whole mass having been turned over by the salmon during the process of depositing the ova. The small stones on the surface of the

bed seemed larger than those around, and much cleaner on their surface, and a fanciful mind might readily describe this appearance as a sort of pavement; of course there is not the slightest ground for an expression of this kind. At a depth below the surface of the gravel above described, varying from nine inches to twelve, hundreds of ova of the usual size were turned up with the spade, on the 25th of February. They were clear, transparent, and seemingly unchanged. We know from other facts that the depth of the deposited ova may be in some instances twice that stated, or about two feet below the surface of the gravel; but on what this may depend is doubtful. The ova taken up by me in 1830 were in a similar stream, and at a similar depth; but the stream was narrower and deeper, and the stones occupying the surface of the gravel much coarser. Thus the ova had remained 116 days without any visible alteration or change. The winter was considered one of the mildest ever observed in this country, there having been in some measure neither frost nor snow. The trout took readily enough with the fly (25th February), and indeed there were a few natural flies on the river. We took two dozen of trout, which were in very good condition, particularly those of a small size. The stomachs of these trout were full of small insects, as beetles, screws, &c., as they are called by the country people, larvæ of flies, and cod bait generally, with which the gravel of the stream abounds in an incredible degree. In the gravel-bed the ova of salmon and trout lie safe from every living enemy, and in the midst of a profusion of food whose habitat is the same as their own, and whose progress of incubation and subsequent rise through the gravel is quite similar. The bed of the river, then, is the soil which furnishes at the same time the matrix for the young salmon and trout, and the secure and ample supply of food for the young of both species of fish. The great variety and quantity of these insects, together with the depth of their situation, (for the spade which took up the ova was also full of

them,) was truly surprising, and to me inexplicable. I should presume it probable, that as these larvæ of insects, for such they mostly are, rise above the gravel to assume new forms, they offer ready food to the trout and salmon fry of all sizes which may be in the river; but I do not suppose that these fishes dig under the gravel in search of food at any time. Of the ova, taken up a number were brought to town with the utmost care, some preserved in moss and others in water. The journey occupied about six hours, and being made in a carriage, and all possible care taken of them, it is difficult to assign a reason for the death of these ova, which took place without exception, for none of them ever became developed. Two circumstances might affect them: first, the agitation and shaking; and, secondly, the change in the nature of the water. A very fine collection of ova, taken from the Kale, and sent me about the same time by a friend, shared the same fate; they were brought to town very carefully, but closely packed in a large phial in water, and by coach. In the mean time, the ova left in a house situated on the banks of a river, from the bed of which they were taken, lived and became developed. I may here remark the following very singular circumstances. It is said by Sir H. Davy, on the authority of a person of the name of Jacobi, whose writings I have not met with, that the ova of salmon are deposited in the gravel of rivers under streams, in order that they may be perfectly aërated or exposed to water which is so. This reason, which appears so plausible, is probably not the true one. Ova taken from the bed of a river at any time, from January to March inclusive, and not shaken or carried far, will live and become developed, *i. e.* grow to fish of about an inch in length, in a small glass full of water, *changed not oftener than once a-week.* They become developed early or late, *i. e.* varying a week or ten days, according to the temperature of the room in which they are placed. They will appear ten days or a fortnight sooner if placed in a warm room,

than had they continued in the bed of the river; and, after having cast the slough, will live *about ten days* (seldom or ever longer) in water *unchanged*, apparently thriving, growing and darkening in colour (if exposed to the light) every day. But they have not been observed to eat any thing offered them, and they invariably die, whether the water be changed or not, after attaining the length of about an inch and a quarter. That they live and thrive thus in unchanged water, must be the effect of habit. We shall presently find that the grown smolt cannot do so, and that when confined it quickly sickens and dies,—a fact of the most unexpected and extraordinary kind, and perfectly unknown to me, previous to these experiments and observations. I request it here to be understood, that all these remarks are made from personal observation, and are in no shape founded upon hearsay evidence, even of the most respectable kind.

I have already remarked, that, on the 25th March, the spawning bed was again reopened before us: nearly all the young fish had cast the outer shell. Now, it is almost certain that this took place two days before, or on the 23d March. Thus we have 142 days from the deposition of these ova in the gravel-bed by the spawning salmon, or exactly twenty weeks; and though this period may vary a week or two, it will require much stronger evidence than has as yet been submitted to the public, to induce me to believe that these variations ever amount to any thing very considerable. Ova deposited, as has been said, upon the 1st April, would require to be developed before the 1st June, since no one has ever ventured to affirm that smolts are taken, or were ever seen in rivers after that time. Hypotheses of this kind require that the ova of the salmon pass through, in eight weeks, those changes which, under the usual circumstances, require twenty for their perfection; and though this is just possible, it is any thing but proved. Wheat sown in October and in March will ripen about the same time; but when I am told that salmon

may deposit their ova late in May, and that these may be developed in a few days, I reply, that all the elements of a correct observation are wanting to render the statement worthy of any notice. It is true, and I have shewn it myself, that the ova may be hurried on by artificial heat, and that consequently the warmth of April and May may effect the same changes in the ova, and bring them forward equally with those deposited in October and November; but still this must have its limits, and the circumstances under which it is said to take place are totally unknown. Though the young fish have cast their sloughs, and, in the observations now detailed, had done so on the 23d of March, all the ova, however, in the bed had not done so, but by far the greater numbers; so that for one ovum found entire, there were hundreds which had quitted this state, and assumed the form of fishes. Of the fry which had burst the shell on the 23d March, several were removed on the 25th March, and put into a common tumbler-glass full of water, whilst some were carried to town in a small phial. Of six removed in this way, one only reached town alive: it remained healthy and lively for five days; but, so, so soon as a small portion of bread was thrown into the water sufficient to tinge it, the fish almost instantly expired. Those left in a house near the river from whence they were taken, were found, on the 1st April, to be perfectly healthy, and had grown considerably. They had become much darker, and were about an inch in length; a considerable part of the yolk was still external to the abdomen: the water had scarcely been changed, and it would seem that this is not necessary; at all events, it certainly can be overdone, not only as regards the ova, but the very young fry also. It has been observed, that whilst confined in this way they refuse all sorts of food, even that on which we are assured, by positive and personal observation, they ultimately live, after they have been some time at large in the river. I regret that I did not make the experiment myself,

so as to enable me to speak positively of a fact of the most singular nature, viz. that they should refuse all food whilst in a state of confinement. I am inclined to believe that this is the case, and that this is the exact statement of all the observations that have ever been made upon them,—that when removed from the gravel-bed, on their first bursting the shell, they are somewhat less than an inch in length, with the yolk of the ovum very large, and in the usual situation,—that they remain in this state under the gravel about eight days and no longer,—that they then emerge from the gravel, and are seen then for a short time to haunt the edges of the river in shallow places,—that the ova will become developed, if taken from the gravel, and put in water, to all appearance as well as if they had been allowed to remain under the gravel, and, if not exposed to too high a temperature, will pass through the same changes, in all probability, in the same period of time. I do not believe, however, that in the latter circumstances (*i. e.* in confinement) their life can be prolonged above *ten days* from the time of their bursting the shell; at least, this I have myself put to experiment, and all persons to whom I have spoken on the subject agree with me. In accordance with these remarks, it was found, that, on reopening the spawning-bed on the 1st April, or in about eight days from the time of the bursting of the shell, by far the greater number had evidently already quitted the gravel, so that only a very few remained; in fact, there was not one for hundreds which existed there the week before. These had attained the same length as the ones kept in the house during the interval, but the latter were darker in colour, in all probability from the exposure to light. The temperature of the gravel-bed was 41° at 9 o'clock A. M., that of the water about two degrees higher. At 12 o'clock the temperature of the water had risen to 45°, and of the air to 55°. The temperature of the gravel-beds during winter is probably seldom under 39°, but we speak from rather a limited observation as to this matter.

We were now anxious to learn if the fry, whilst under the gravel, or shortly after their escape from it, were exposed to destruction from the trout inhabiting the same streams, forasmuch as very bold assertions had been made on this as well as on most matters connected with the history of the salmon. Accordingly, on the 23d and 31st of March, days on which we have seen that the salmon-fry in the state of fish might be considered exposed to be devoured by trout burrowing under the gravel for that purpose before the escape of the salmon-fry, or when, just escaped from the gravel-beds in a feeble and helpless state, it might be supposed, we repeat, that at this particular time it would be found, that trout fed upon these young salmon. Now this most assuredly is not the case. About three dozen trout were taken on each of those days, and all opened purposely to ascertain the fact, but their stomachs were uniformly found full of aquatic insects, and in no instance could the slightest remains of the salmon-fry be detected as the food of the trout. It is needless to remark, that trouts of various sizes were opened, lest it might be peculiar to trout of a certain size to attack the fry in that condition.

On the 20th April 1832, these rivers were fished with fly, and were found full of salmon-smolts, varying from six to nine inches; such being the rapidity of their growth, from the 1st to the 20th April, or in about three weeks. They were in the finest possible condition, covered with small silvery scales, differing in shape (I mean the scales) from those of the trout or par. They frequent the still water towards the foot of pools, and feed exclusively on flies, cod-bait, and aquatic insects, during the whole period of their residence in fresh water, their food differing in nothing from that of the common trout.

It is almost unnecessary, I hope, to remark, that the stomachs of the fry were examined with great care by myself, and their contents minutely inspected. They have not been observed by

me in any state as fry in the stomachs of trout or kelts, *i. e.* of spawned salmon.

They are of very rapid growth, many attaining the length of nine inches in twenty-seven days, supposing that I am correct in the exact period of their appearing above the gravel; but during the first seven days, whilst living on the yolk, they grow very little; thus, in twenty days, they apparently grow from one inch to nine inches in length.

When dissected they are uniformly observed to be healthy: a few small round parasitical worms infest the swimming-bladder, and occasionally, but very rarely, I presume, the lernæa may be seen on the gills; but they are uniformly in excellent condition.

They run rapidly into putrescence, and differ entirely from the parr and common trout in this respect, so that it is scarcely possible to mistake them for each other, if this be attended to. The form of the spleen in the par differs so much from that of the river-trout, and smolt and full grown salmon, as to shew, I think, specific differences; but it resembles very much that of the herling.

It ought to be remarked, that the weather had been, during the whole of the spring season, dry, with sunshine, and scarcely any rain had fallen. Of the smolts taken on the 20th, 21st, and 23d April, an attempt was made to preserve some in a large basin, in which the water taken from the same river was frequently renewed,—nevertheless they died in a few hours, and this took place even though replaced in an isolated portion of the river. It seemed, indeed, that when taken with the fly, however gently treated, the smolt generally died, though scarcely removed from the river. They will not in fact bear the slightest handling. On the following day (23d) we used a net, avoiding as far as could be the possibility of injuring them in any way; yet the result was the same,—they constantly died in a short time after being touched. We are filled with surprise when we read of smolts caught (it is

not said how, *but it matters not*), and after being, according to all accounts, rather roughly handled, and even mutilated by the amputation of a fin, replaced in water, and arriving at mature years as full grown salmon; we are, we repeat, lost in wonder at the amazing contradictions between such observations and those we have personally made, observing every possible care. We learned at this time that an attempt had been made some years before, to remove salmon-smolts from these rivers, with a view to the stocking of a pond, about six miles from the banks of the river; and though the ample fortune of the noble proprietor enabled him to make the experiment on an extensive scale, by removing smolts considerably grown, in large buckets of fresh water, and that water constantly renewed, carried also on men's shoulders, that the shock sustained might be as little as possible; the result was, as we have every reason to believe, an entire failure. It is possible, however, that localities still more favourable may have given a different result to the experiments of others.

On the 5th May 1832, the same rivers were again fished. Smolts still abounded, notwithstanding considerable rains had fallen, followed by a high flood of the river; but they had undoubtedly descended the river about two miles and half, *i. e.* to deeper water, so that near the spawning ground few or none were to be found; whereas trout, not however in good condition, abounded. These smolts were generally about the same size as those taken on the 1st of the month, and I doubt not belonged to a later set.

As the observations now detailed were made on rivers joining the Tweed from the north, and on rivers whose course was much interrupted by wears and other impediments, to the ascent of the salmon upwards, and the escape of the fry from the tributary streams to the Tweed, it was thought right to change the site of observation, and observe for a few days the state of some salmon streams joining Tweed from the south. This was

done between the 29th April and 4th May, but nothing was observed contradictory of the remarks already made. In the Yarrow river, which we fished, as differing from those already examined in the circumstances of arising as a uniformly large river from a lake of considerable magnitude, a few smolts were taken in the river shortly after its quitting the lake. The fry was rather scarce, and trout still more so; the fry were scarcely the size of those taken near the sources of the northern streams fourteen days before.

In the streams which join the Teviot, and more especially the Kale, an admirable trouting and salmon stream, the fry abounded to a very great extent, and were collecting in mill-dam heads preparatory to quitting the river.

Previous to the 2d May the fry were found to have quitted the College Water, which rises near the base of the highest Cheviot; but fry still about the same size, *i. e.* from seven to nine inches, abounded in the first pool of the Glen, a name which the College Water assumes on its joining the Beaumont. This pool was formed by a mill-dam head: the young smolt were found in this dam-head in vast abundance. At Horncliffe on the 4th May, the Tweed was reported to abound with salmon smolts; but we merely report the fact as it was stated to us by the fishermen. We saw none taken in the nets, though our observation as to the management of these nets for many hours was minute and careful. The result of these observations, with the mode of fishing the Tweed by coble-nets, (observations to me unexpected, and leading to the most important results in an economical point of view), fall to be considered more properly under another section.

I have thus described the generation of the smolt in fresh water, near the source of a tributary stream of one of the finest salmon rivers which exists in Britain. Of the further progress of the smolt in its growth from its extreme length, so far as I have

observed, of nine inches, whilst frequenting the upper streams of fresh water rivers, until its growth in the sea to the size of the grilse, and its return again into the fresh waters, we really know nothing. Many excellent observers, and amongst others my friend Mr Buist, consider this part of the natural history of the salmon to be perfectly made out. I can assure him that there is not a single authentic observation on record respecting it. The stories of fin cutting, notching, marking, &c. of the smolt, and the return of these same marked fish as grilse or salmon, are, so far as I know, beneath all criticism. There is nothing in their history, which in the slightest degree merits the name of authentic; and we take the liberty of requesting the special attention of practical fishermen to this circumstance. Having described the spawning of salmon towards the sources of clear mountain streams, many might imagine that, agreeable to *my* opinion, they can spawn no where else. Now, in point of fact, I entertain no such theory; but having no proper series of observations to proceed on, as no such indeed exists anywhere to my knowledge, I cannot say to what extent salmon may spawn in brackish water, or within the reach of the tide-way, nor what may become of the ova so deposited. On this, as on many other points in the natural history of the salmon, we require not general opinions, founded partially or altogether on hearsay or conjecture; but a "journal of observations," conducted by a person competent to the task, devoid of all local interest, connected neither with river or coast proprietors, a person, in short, whose sole object is the investigation of truth. The journal of an individual of this kind will stand the test of the investigation of such a Committee of the House of Commons, as sat during the years 1824 and 1825, whose questions prove *them* competent to investigate and judge; and we will take it upon us to say, that unless such steps are taken, no other kind of evidence will ever stand the test of a similar Committee in any future attempt which

may be made to legislate on this important and national question.

NATURAL ENEMIES OF THE SALMON.

We have examined the contents of the stomach of trouts of varying sizes, caught by ourselves in streams and rivers inhabited by the fry; and of the many opened for this special purpose, we have never found the slightest vestige of salmon-fry or smolt in their stomachs. We have taken trout at the time that the fry abounded in the river in its smaller condition, and still no vestige of fry appeared in the stomach of these fishes. That trout devour the ova of their own species, I am perfectly aware; but to get at the salmon eggs, trout would require to dig up the gravel-bed: now, from the details already given, this seems to me the most improbable of all things. Trout at the season when fry first appear in the stream, do not seem disposed to take minnow, or small fishes of any kind; and the amazingly rapid growth of the fry renders it impossible that they should serve as food for trout, at least for any length of time. Three kelts have been examined by a person on whom I could entirely depend. Their stomachs were found full of the usual food of trout, that is beetles, aquatic insects of all kinds, and larvæ of flies or cod-bait. Now, this was when the fry were just making their appearance in the stream. During winter, when the spawned salmon are in the rivers, they do not seem inclined to eat any thing; and trout are much in the same condition; and though it be true that salmon roe is considered as a good bait *, both for salmon and trout, yet it does not follow, that they themselves are enabled to obtain it. No statement, therefore, rests upon more meagre data than that trout and kelts are the natural enemies either of the ova or of the fry of salmon.

* Do they know it as such?

Man himself, then, is the greatest enemy of the smolt. Some have thought that by floods the fords or shallows containing the spawning beds may be entirely removed or covered with a new load of sand and stones, and in this manner thousands of ova for ever lost; but there is not on record any properly authenticated fact of this kind. Sudden changes, no doubt, do take place in the beds of rivers; but, so far as my personal observation extends, I feel inclined to say that these are not very common. The same streams and pools observed many years ago, I find in most instances to exist; and so far as I have observed, the changes which do go on in the beds of rivers are rather gradual than sudden. The interest of the neighbouring proprietors are generally hostile to such changes, and usually oppose it to the utmost by breastworks, &c., whether the ground be valuable or not. Mr Fraser, a practical fisherman of great experience, has taken notice of the king's-fisher, eels, flounders, &c. as being natural enemies of the salmon fry; but it is easy to understand that nothing of all this is proved, and indeed every day these natural enemies are reduced by the investigation of some naturalist. The flounder for instance, does not seem to me much to leave the brackish water; and it has not been any where proved that any of the salmon species breed in this locality.

The spawning of salmon ceases, I should think, in all rivers in or about the end of February *. Experienced fishers have

* The following statements, derived from the Evidence before the Committee of the House of Commons, are founded on tolerable evidence:

Wales (Brecon).—" Has seen many spawn in the middle of February, never saw a fish spawn in March."

Cork (Lee).—" Spawn in October and November."

Counties of Cromarty and Sutherland (Shinn, &c.)—" Salmon spawn early in the month of October, all the breeding fish have passed up, and before the end of November the spawning is over."

Inverness (Beauly).—" They spawn the latter end of October."

Aberdeen (Don and Dee).—" They have all done spawning by the 1st February."

Berwick (Tweed).—" Salmon spawn in January and February."

indeed asserted that hundreds of unspawned fish may be found in the end of March and beginning of April in the Tweed as low as Kelso; I imagine that they are in error. Kelts taken in April and May might pass for unspawned fish with some to whom the anatomy of the organs was not familiar. I have taken, and seen taken, in the Tweed at Clovenford, female salmon on the 25th April, and have known them taken a few days later with the ova very highly developed, so that they might easily have been mistaken for unspawned fish: they were merely female kelts which had remained long in the Tweed, and its tributary streams, and were on their way to the ocean. Persons not familiar with animal structure may very readily be deceived in these respects. I reckon their testimony and observations of little or no value. The kelt feeds and grows much stouter, and with the growth of the other parts the roe and milt again begin to be developed. Many salmon spawn early in October; now it has not been proved that they necessarily hybernate like trout, but rather return to the tide-way with the first floods. Salmon spawned so early, may very readily recommence their migration into rivers in prime condition with the first flood of January and February. Moreover, great numbers do not get into the spawning condition; but they deteriorate constantly by a residence in fresh water *.

During the months of March, April, and May, the rivers abound with *kelts* or spawned salmon descending towards the ocean, and smolts or fry pursuing the same course. The migration of salmon in prime condition into the estuary, and a short way up the rivers, continues, and become, as summer advances, more frequent; but I will not readily believe that many of these salmon would early in the season push high up the rivers so as

* The great cause, however, of the deterioration of the salmon, is his *disinclination* to feed during the rapid growth of the organs of generation. A barren fish coming into fresh-water would not, in all likelihood, fall off so soon as the fish about to spawn.

to remain there. A return of the salmon and salmon-trout caught by a series of stage-nets, or even by the coble-net, in that part of a salmon river, extending from the point, influenced by the highest flood of tide to the lowest ebb, characterising those numbers taken during flood from those during the ebb, would settle the above and many other points; but the Committee of the House of Commons found it impossible to get at such data; in my opinion they do not exist, but their value would be very great. Salmon do not begin to ascend rivers much beyond the tide-way, until they commence altering in condition. Hence, it may be said to be the month of July or August before salmon in any numbers ascend the rivers beyond the tide-way, their number increases in August and September; the causes of this, together with the daily deteriorating quality of the fish need not be stated. As a reply to all doubts on this point, it may be affirmed, that were the Tweed the property of one man, he would fish it from Berwick Bridge to the sea or bar of the river, and no where else. And, on carefully observing the Tweed fishings, it has often filled me with astonishment that the proprietors in a body do not adopt this method. The saving of expenditure would be enormous, and an effective plan for preventing the destruction of salmon at improper seasons adopted. But about this time, that is in December, January, February, and March, a few salmon are likewise found in high condition, with the organs of generation very little developed. These are fish which had either not altered into the spawning state during the preceding year, a very common occurrence, as we shall find, as well with salmon as with trout, or which had spawned very early, and returned as early to the ocean, have recovered their good condition, and resumed their usual migratory habits of ascending rivers, and that to a very limited extent, during floods.

The presence of these in the rivers has done immense injury to the salmon fisheries of Britain, and has caused the loss of thou-

sands of unspawned fish. The river Eden, for example, is said to be what is called an early river, and the river Lee at Cork is, or was lately, fished throughout the whole year.

Izaak Walton observes, "that as there is no general rule without an exception, so there are some few rivers in this nation, that have trouts and salmons in season in winter, as it is certain that there be in the river Wye in Monmouthshire, where they be in season, as Camden observes, from September till April." To which Mr Rennie adds in a note: "In the river Lee, which runs into the sea at the Cove of Cork, salmon are likewise in season the whole year round, as I can myself testify, having resided at Cork the greater part of a year. (Signed) J. R." A dangerous error is conveyed by language of this kind; the tacksmen of numerous lakes and rivers in Britain, have constantly endeavoured to shew that their particular locality furnished clean salmon all the year round. The object of this was to get permission of the Legislature to fish these localities, and to sell their fish, and thus to command a kind of monopoly of the market, calculating that with the few sound salmon, which we presume may be found in and about the mouths of most rivers, or as high as the tide-way, throughout the year, to have it in their power, in the first place, to obtain a high price for a comparatively rare article, and at the same time to introduce, by means of kippering and pickling, &c., a vast quantity of unwholesome food, which they of necessity take along with the few sound fish. The fatal error, I repeat, is, that in finding sound salmon in the *mouth* of a river throughout the year, the observer merely takes note of this fact, without at all considering the mischief done by the taking of unspawned or foul fishes along with them; all the elements of a correct observation are thus wanting, or at least of one which may be practically useful. The first object in a question of this kind on the part of the Legislature, being not the taking of a few sound fish during the winter and spring months, for the gratification of the

appetite of a London epicure, but in what way the fishing can be conducted with least detriment to the public in general? It is not pretended to be denied here but that the spring fishings are very valuable to those who possess them; and, in order to avoid any unnecessary alarm on their part, a mode of taking these sound fish without detriment to the public will be shewn under its proper section.

APPENDIX TO PART I.

APPENDIX A.

1. *Period of Spawning, and Use of unwholesome Salmon as Food.*

The usual period for the spawning of salmon, has been established on pretty clear evidence. The month of November seems to be the month in which the greater number spawn; but it may also be admitted, that throughout the greater part of October and December, this process is going on in all salmon rivers. It were a matter of great moment agreeably to the existing regulations respecting fence-time, that the precise period when the process commences, and when it ceases, were fully determined, and put by intuitive evidence beyond the reach of doubt, and the conjectures of interested persons. To determine the period when the greater number of fish in salmon rivers shall have ceased spawning, for if it really be as may be suspected, that those which are said to spawn in January, February, March, and even so late as the 6th of April, be few in number, then though the fact be an interesting one in zoology, it becomes unimportant in a legislative view. Were it even proved, which it has by no means been, that fish do spawn so late as April, it does not at all follow that the ova should come to maturity. There must be some limit to this. In a natural state they seem to require twenty weeks, this being as I have shewn the usual period they lie imbedded under the gravel; and it is not to be believed, except on the most unquestionable authority, that the salmon egg can pass through the same stages in a few days, requiring, under other circumstances, and those the normal or usual phenomena, twenty weeks for its development. A negative proof, but an important one with regard to this, is connected with the fact, that salmon smolts of the usual size when taken by angling, are scarcely if ever seen after the month of May. My friend Mr W——, a most successful angler, informed me that he had seen them on one or two occasions in June in the Tweed as high up as Kelso, but the season had been remarkable for drought, and these smolts had acquired an unusual size and weight, being in some instances ten inches to a foot in length, and many of them nearly one pound in weight.

As salmon approach the spawning condition, they are understood by most educated persons to be unwholesome as food for man. The same objection lies to the use of the kelt, *i. e.* the salmon after having spawned. As both these positions are universally denied by the peasantry and lower orders of society *; the salmon are slaughtered

* "You may get people to buy any thing if you sell it cheap enough." Mr J. Johnstone's Evidence, p. 53. Every thing I have observed with regard to the use of unwholesome food amongst the lower orders, tends to confirm Mr Johnstone's opinion.

With reference to this note, it may be suggested, that the pickling and kippering of salmon by

nearly to extermination on the spawning ground, and the fish used as an article of food almost universally. So active is this search for the spawning fish, and so efficient the means and instruments of destruction, that it appears to me incredible that any should escape. Nevertheless by secreting themselves with infinite care, aided by the muddy state of the streams in winter,—by the inclemency of the season, interfering a good deal with the lounging of poachers and idle persons on the banks, the smaller kinds of them do escape in considerable numbers, and the banks of a very considerable number of the tributary streams of great salmon rivers, being in many instances now nearly uninhabited, even a few large fish succeed in depositing their spawn; but reappearing in the stream in spring as kelts or spent fish, they are once more pursued, and are again taken to extermination if possible. Their condition during the winter *in rivers* must be viewed as a state almost approaching to hybernation. They lie nearly stationary for weeks when they find the place gives security. They eat nothing, so that their condition seems to me to approach closely the state of hybernation. Our personal experience with regard to the feeding of the kelt, is certainly not extensive; but when the melting of the snow and spring rains swell the rivers, he seems to begin to move about, and takes the food at that time to be found in rivers, and, like the smolt, may feed very actively on his journey to the ocean. The only admissible proof of this, viz. the inspection of the contents of the stomach of the kelt under a variety of circumstances by a competent person, is wanting. The observations should discriminate the true salmon from the salmon-trout, &c.

2. *Hybernation of Trout.*

My attention to the probable hybernation of the trout during the winter months, was first called by my friend Mr A. Darling, and afterwards by Mr Walker. Mr Darling, to whom my obligations throughout this inquiry have been very great, mentioned to me that in clearing out some drains and well heads, communicating with the running stream of the river, and during which operation the workmen had to cut down not only the bank of the drain composed of soft mud, but also to dig into the gravelly and muddy bed of the drain itself, they found imbedded in the soft materials, I have just mentioned, vast numbers of trout of various sizes, shewing little signs of life, until put into the stream. Parts of them seemed as if frozen. This fact was observed by a number of persons often, and can be repeated. This habit may be confined to the spawning fish, which I have found by no means includes all the trout, whether large or small, in a river. I shall here insert a short extract from my journal of observations, which will best explain to the naturalist my meaning:—" Of fifteen trout, varying from 9 to 14 inches, taken on 23d September, by a net, in the Annan, at Hallbeath, and after a long course of dry weather, there were *eleven* of the largest, in whom the organs of generation had undergone no increase whatever; so that it was quite evi-

improper persons, is a subject well worthy the investigation of the Legislature; as this is the only means by which positively unwholesome salmon can be imposed on the public, and is at the same time a strong inducement to poaching.

dent that these fish would not have spawned during the present year. The other four had the milt and roe very large. In the males, in which the milt was a mere vessel, the lower jaw had not undergone any change. The trout was thin, but in good condition. The flesh of a pink colour." On the other hand, my friend Mr Walker, without being acquainted with the circumstance made known to me by Mr Darling, and in a totally different locality, mentioned to me, that, in removing some of the eggs of the salmon from the bed of the Kale, the person employed turned up with a spade from the bed of the river, and at the depth of 12 to 14 and even 18 inches, a considerable number of trout of varions sizes. These lay imbedded in the gravel, placed below a running stream. They seemed at first dull and confused, but in a little time swam away into the stream and disappeared, and, as Mr Walker thought, again buried themselves under the gravel. These facts seem to go far to explain the vast diminution in number of trout and other fishes in rivers during winter.

I have known an instance of at least 150 salmon remaining in and about the sources of some very small streams and pools, in so narrow and contracted and exposed situations, that had they not been extremely well concealed, they could not have escaped the surrounding peasantry. So soon as the spring advances somewhat, those salmon which, during a season of almost unexampled drought, had remained securely concealed in these paltry rivulets, having appeared in the principal channels of the rivulets, were immediately discovered, pursued, and taken; many of them must, I think, have been concealed amongst the mud and gravel. Of trouts, it must be known to every angler that during spring, after each fresh or flood, great numbers appear in the stream in ill condition; these come from their places of concealment, as I have described.

3. *Experiments respecting the Growth of the Salmon Ova, Smolt, and Grown Salmon.*

There are two questions which persons of sound judgment and of great experience with regard to the salmon question, still think undecided, or at the least admitting of further illustration, or of demanding a more extended proof. The first of these is a series of experiments required to determine the growth of salmon-fry, from the state of the egg to its attaining the length of 6, 7, 8 or 9 inches, before which it is seldom seen by the angler, and after which it ceases to be found in fresh water rivers; *secondly*, a second series of observations is required to prove that the fish we call salmon-fry, taken in salmon rivers so readily by angling during the months of April and May, do really proceed to the ocean, to return to the rivers, after a period, as *grilse*, *salmon-trout*, and *salmon*. With reference to the *first*, I would observe, that the series of observations contained in the text, will be fonnd, when candidly weighed, to go far to decide the question. It is true that Mr Buist of Perth, the most experienced of all those writers I have met with in regard to salmon and their fisheries, and the best qualified to judge of these matters, still has doubts on this point; and it is easy for any person viewing the question in the same philosophic and accurate manner in which Mr Buist does, to perceive, as he has done, that the proof by intuitive evidence is wanting, a deficiency which no series of disjointed observations, however well conducted, can entirely compensate. All I shall say on this point is, that, on reviewing the locality and

circumstances under which my observations were made, I have every reason to think the conclusions legitimate. The broad and deep streams of the Tay near Perth, do not seem to me favourable for experiments of this kind, and, by their very extent of waters, widens the question, rendering it of course every moment more and more complex. On the other hand, the narrow stream in which my observations were made, proportionally, as it were, contracted the question, and brought it more within my reach. Still it is true that no one has succeeded in observing the development of the egg, and the growth of the smolt, to the length of 6 or 7 inches; or, in other words, proving, by a direct experiment, performed in vessels placed under their immediate observation, that the fishes abounding in salmon rivers, during the months of April and May, are really salmon-smolts, and proceed from the ova or salmon-eggs deposited under the gravel of these rivers during the preceding winter.

The second question of difficult solution is that regarding the return of the fry from the ocean to the salmon river, and in what form, and at what period, the salmon does first return from the sea: I have already remarked, that upon this question there is not a single authentic observation.

4. *Alteration of the Nature of Rivers by Drainage.*

The system of drainage practised over the Cheviot and Lammermuir mountain tracts is very extensive, and must have considerably modified the nature of the rivers connected therewith; but whether this really influences the deposition of the salmon ova may be questioned: for it is worthy of remark, that at the period of the year when the salmon is busiest in the propagation of its species, the rivers and tributary streams are generally in their very smallest condition; and hence, and also for other causes, it must, I think, generally happen, that the salmon select, almost in an unerring manner, the never-failing stream under which they deposit the egg. All that has been said by ingenious persons about the destruction of salmon ova, in consequence of the drying up of streams, is very plausible and ingenious, but without foundation in positive personal observation. The difference between ingenious conjectures of this kind, and the results of personal positive research by correct and authentic observers, is but little understood by many persons, who, nevertheless, think tolerably well upon ordinary topics. These persons seem incapable of that careful mental process which requires us to go back and inquire into the sources of our information and opinions.

5. *Can the Supply of Salmon be increased? Has it diminished?*

The supposed abundance of salmon in former times and their supposed scarcity now, seems to me a mistake.

1*st*, An occasional scarcity of salmon proves nothing; nor even a lengthened scarcity. In the vast waters of the Columbia, there is said to be a scarcity every second year *, of a fish much resembling the salmon both in habit and quality as food. But the undue fishing or poaching of the tributary streams of the Columbia, the taking of smolts in too great quantities, the evils occasioned by mill-dams and manufactories, the in-

* Travels to the Banks of the Columbia, by Mr Ross Cox.

fluence of stake-nets against "the interests of the upper heritors" of fishings, cannot even be imagined, in respect to the Columbia, by the most fantastic mind.

2dly, When salmon abounded in the remote districts of Scotland and Ireland as an article of food, the value of the London market had not been discovered, or, if known, could not be reached; steam conveyance and the use of ice were unknown; the accumulation of wealth in London, and in the empire generally; the vast increase to our population, did not exist. The use of grouse and venison, and claret, as articles of food, were perhaps once as familiar to the lower as to the wealthier orders of society; nor is it imagined that these articles of food and drink have become scarcer, though they be now interdicted to the poorer classes. As with these, so it it is with salmon; the whole state of society has changed within twenty years, and science has altered every thing. To suppose that salmon, whose breeding ground is to a certain degree limited, and much interfered with artificially, could be made to increase indefinitely, is at least a strange supposition; and to imagine that a much prized article of food which thirty years ago could not have been forwarded to the great national market London, during a great part of the year, without running almost certain risk of spoiling, should now be brought into the smaller and inferior markets, and sold at a price suited to the means of the mass of the inhabitants of small towns, is quite unreasonable, and what will assuredly never again happen.

In a word, the farmer, and the inhabitants on the banks of rivers generally, in former times caught, as they in fact still do, towards the close of the year, an enormous quantity of unspawned fish, which they kippered and prepared in a variety of ways. By taking the kelts in the spring they added to this stock of unnatural, and no doubt most unwholesome, food, on which they fed their labourers and apprentices throughout the year.

It seems to me an agreeable delusion, but still a delusion, that as salmon were once so abundant as to be used as an article of food by the peasantry and poorer orders of society, so, by wise regulations, this state of matters may return only, I should suppose, with the loss of all the lights and knowledge of civilized life acquired in the interval. At that time, the voyage from Scotland to London averaged three weeks or a month; now it is accomplished in forty-eight hours. What conveys men, will carry salmon to market. Ice packing has been in use only within these few years; and extensive pickling on the spot where the fish are taken, has become quite common. The persons who maintain the delusion I speak of must have forgotten all this; salmon is fast becoming a part of the game of the land, and laws for its protection as game will probably soon be enforced.

6. *Observations to determine the Comparative Advantages of Sea and River Salmon Fishings.—The rights of Proprietorship in Salmon, and to whom naturally that right belongs.—An attempt to show the exact Limit of a River Fishing, calculated with a view to the preservation of the Salmon, and the interest not only of the Proprietors of these Fishings but of the Public.—A plan to extend Sea Fishings all over the Empire. On the necessity of a total change in the Mode of Fishing most of the Salmon Rivers in Scotland.*

This extended section has been omitted at the request of several members of the Royal Society, who have suggested that its vast importance renders it a fit subject to

be addressed to the Highland Society of Scotland, or to the Legislature, rather than to a society whose objects are purely scientific and literary.

The only observation that is thought necessary to introduce here is, that the author of these papers trusts, that every one adequately informed as to the nature of salmon fishing, will agree with him in thinking the measures adopted by Parliament, at the suggestion of Mr Home Drummond, with reference to the stake-net fisheries, to be most prudent and cautious, and that whilst most of the great questions remained unsettled, and more especially since none was found throughout their most extended "Inquiry," who could offer a rational conjecture (founded on facts), *personally* known and understood; (*the result of positive research by a competent naturalist and physiologist*), as to the food of the salmon, its *habitat* whilst in the ocean, and its feeding ground, it would have been imprudent to risk extensive experiments on the fishing laws.

7. *Opinions of Authors, including the Evidence and Report on the part of the Committee of the House of Commons.*

The latest opinion worthy of any notice with which I am acquainted, is that of Professor Rennie, Professor of Zoology, King's College, London; and contained in a new edition of the Complete Angler of Izaak Walton, published August 1833.

Walton, in speaking of the Fordidge trout taken in Kent near Canterbury, says, "It is accounted the rarest of fish, many of them near the bigness of the salmon, but known by their different colour, and in their best season they cut very white, and none of these have been known to be caught by the angle, unless it were one that was caught by Sir George Hastings, an excellent angler, and now with God, and he hath told me he thought that this one bit not for hunger, but wantonness, and it is rather to be believed, because both he then and many others before him have been curious to search into their bellies what the food was by which they lived, and have found out nothing by which they might satisfy their curiosity." To this Mr Rennie appends the following note: "The same is true of the salmon, which has never any thing besides a yellow fluid in his stomach when caught. The same is also true of the herring."

This opinion must be quite peculiar to Professor Rennie. I know of no author in which such a fact is mentioned, nor have I ever seen any thing of the kind myself. I mean the yellow fluid spoken of by Mr Rennie.

The late Dr Walker, who for his time was a good naturalist, and undoubtedly a most careful observer, possessing the requisites of an accurate and scientific mind, viz. accuracy of observation and fidelity of narration, read some papers to the Highland Society of Scotland, on the natural history of the salmon and of the herring.

His account of the samlet, or smolt, is perhaps sufficiently accurate for general purposes; but where the salmon, a fish of such value, is considered, the history of the samlet if possible ought to be more faithfully, or at least more minutely, recorded. This I have endeavoured to do in the text, p. 472. The herling he considers as a samlet of the same year, returning for a short time to the rivers in August and September, but never ascending far up the river; twenty-six miles he considers as a long way up for them to ascend. After a short residence they return again to the sea, and are not to be seen in the river from September till the following April." P. 354. In respect to these

opinions of Dr WALKER, I must observe, that, according to my own experience, they contain numerous and serious errors. Herlings may be taken in the Annan during all the winter months; and, in respect to their spawning, a process, he thinks they do not perform, a reference of the reader to page 465. of the text will be found to set the matter entirely at rest.

He says, moreover, that in the Tay it is called the Lammas Whiting, and that it abounds in every salmon river. The next year "it becomes a whiting or white trout:" he seems to think that neither of these fishes spawn, "the grilse is another year of the same fish, and he says it is never known to spawn," nor to ascend distant rivulets. (P. 357). The errors contained in these latter opinions of Dr WALKER, will be found fully explained in various parts of the text and appendix of this memoir. Mr DRUMMOND, an excellent practical writer in the same work, (Transactions of Highland Society. p. 365), says that "the nature of the food whilst in the sea is unknown."

The Rev. Dr HEADRICK, who made many excellent suggestions respecting the fisheries, remarks, (p. 444), I "suppose they (salmon and herring) live chiefly on water and on small insects, which abound both in the sea and rivers. I have been told of the fry of smaller fishes found in the stomachs of salmon; but such instances never occurred to me, and I never heard of any animal being found in the stomach of a herring. Mr HEADRICK's opportunities were ample since he examined the matter personally, and undoubtedly was present at the fishings in Lochbroom, and indeed on both coasts.

Some of my friends have thought, that I was not bound to notice the Parliamentary Report on the Salmon Fisheries, but simply to have brought forward what I had myself discovered or elucidated. I feel at a loss to understand how they had arrived at this conclusion The Parliamentary Inquiry resolved itself into an inquiry respecting the natural and economical history of the salmon, and might be expected to contain all that was known on the subject.

Shortly after the printing of the evidence before the Committee of the House, various journals announced the opening up of these scientific questions by the Committee, and I naturally felt anxious to learn the sources whence they had drawn, and the individuals to whom they had applied for information. I was not myself aware of the existence of any researches into the natural history of the salmon, salmon-trout, parr-trout, corregonus, &c. which had any authenticity, or which in any shape merited the name of a scientific inquiry. But still, as sometimes though rarely happens, it might have happened that original experiments and observations had been privately instituted and that knowledge existed of a correct and authentic nature, which had yet been withheld from the public. But discovering soon that the evidence was conflicting and absolutely devoid of all that care and precision requisite to be had in view in scientific inquiries, I regretted much the not having committed to paper some observations I made many years ago on the generation, more especially of the osseous fishes, the repetition of such experiments and observations being very expensive and troublesome; but I regretted in some measure my having been induced to pursue the history of the generation of cartilaginous fishes in preference to the osseous. Sensible now that the inquiry, if it had been happily directed towards the latter, might have gone far to settle most points in the natural history of the salmon, the inquiry having been conducted at a time when

anatomy as a science formed the sole object of my pursuits, to remedy if possible these omissions, I have taken every opportunity to investigate the various questions on the spot, by repairing, when practicable, to the salmon-rivers, exploring them from their sources to their conflux with the sea; by looking into every thing myself, and as seldom as possible trusting the fishings of these rivers to others. Notwithstanding these efforts, which were attended with a good deal of inconvenience, particularly in the examining the state of the rivers at a distance from town, it cannot but be that many errors must have crept into these memoirs. As the whole question is with me strictly a scientific one, and being no further interested in the numerous difficult problems it embraces, than as regards the investigation of truth upon a physiological question, the memoir will be found not so much extended on some practical points as perhaps the public desired.

APPENDIX B.

Evidence and Report to the House of Commons on the Salmon Fisheries of the British and Irish Rivers.

It is in evidence on the part of Sir H. Davy, that he considers six weeks sufficient for the development of the salmon ova, *i. e.* from the period of its deposition until its appearance in the stream as a fish. This would bring the smolts into the stream at a time when there was no food for them. Moreover, the opinion is refuted by the most positive intuitive evidence. Every science must have a basis. Now, the question of the generation of animals is the most obscure and difficult of all questions in physiology, a science resting entirely for its basis on anatomy. Sir H. Davy being unacquainted with these sciences *, was not competent to make observations on the mode of generation of the salmon.

In a succeeding passage of the paper I now remark on, the editor observes, that "in August and September, a fish exactly resembling the young salmon in form, and from ten to fourteen inches long (called Whitlings and Whitings), without visible ova or spermatic secretion, are found in salmon rivers, a mile or two from the sea, and which return to the sea without attempting a further migration." P. 145.

To what species of fish Sir H. refers, in the passage above quoted, I cannot imagine. Does he mean that in the young salmon, fourteen inches long, the sex of the fish cannot be determined, and that the ovaria and milt are not to be seen? Personal repeated observation has shewn me, that in the salmon smolt, whilst yet at the sources of the rivers, and only six inches in length, and previous, of course, to their descent to the sea, the sexes can be distinguished from each other with the greatest ease, the organ of generation being already partly developed. It is by no means difficult to distinguish the sexes in smolts, even without the aid of anatomy, since the lower jaw of the male (as in the trout) is stronger than in the female, and already displays that process pro-

* I take this position as proved by a passage in the Salmonia, where Sir H. Davy speaks of the whale having no swimming-bladder.

jecting upwards, which, afterwards growing to so great an extent, forms a leading feature of the male fish.

The person who could not discover the organs of generation in young salmon fourteen inches long, and who supposed that in fish of that size they are not visible, was likely to fall into other errors; of which not the least remarkable is, the supposing that the projecting and upturned lower jaw of the male (which I find in the smolt six inches long), so distinct, that it is always possible to discriminate male from female previous to their being opened, is so fashioned by the superior activity of the male fish during the spawning act, "which hardens the extremity of the mouth, and bends it into the form of a hook." P. 145.

Notions similar to these have of late years been very extensively spread. They reduce, or attempt to reduce, the causes of animal structure to "animal mechanics," as if animals had originally been made by mechanics. It is a superficial philosophy, founded mostly on low conceits, and calculated to make us believe that the animal structure of each individual animal was particularly framed for itself alone, without a reference to the place it holds in the animal creation; with these persons the spleen passes as a soft padding for other viscera*; the hook-like process of the salmon's jaw is produced by hardening†, or is bestowed on it by Nature for the purpose of fashioning the spawning-bed; and the strong protuberance of the occipital-bone is given to man to protect the head in falling backwards‡. NEWTON, who assuredly was no anatomist, understood enough of the philosophy of animal structure, to perceive that all animals were formed upon one general plan; and HARVEY went much beyond this, since he first pointed out an imposing and mysterious feature in this plan, viz. that most irregularities in human structure were to be found in the lower animals. It was reserved for the present day, and for our own times, to find out that the eyes of the horse are placed laterally, to enable him to kick with effect §, and that *this is the cause why they are so placed*: and that blubber is given to the whale to make him buoyant! why not for man to make oil of? These attempts to reduce the "Science of Life," as physiology has been termed, to a tissue of puerilities and conceits, may perhaps by some be deemed useful, by making it popular; but it is right that scientific men should be informed that such explanations are refuted by the whole phenomena of living nature, and are unworthy the name of scientific explanations.

It were easy to shew here, in the examination of some statements made by practical fishermen and others, that errors and misstatements regarding the most vital questions in the natural history of the salmon, are contained in the Report. The Committee proved, in an undeniable manner, that one practical fisher and tacksman of salmon fisheries of vast extent, was so ignorant of every fact in natural history, that he mistook the tape-worm (a parasite infesting certain parts of the intestinal tube of the salmon) for the food of the salmon. Another also, a practical fisherman, in open defiance of truth and daily observation, asserts that the true salmon lives on sand-eels and fry of other fishes,—a distinguished naturalist talks of salmon going into estuaries in search of worms

* PALEY. † Sir H. DAVY. ‡ "*Animal Mechanics.*"

§ See The Anatomy of Expression in Painting by Sir C. BELL.

and other bait *,—a very extensive practical fisherman affirms that stake-nets have no chambers towards the ebb-flood,—and one of the most extensive practical fishermen of the north, also proprietor of many great salmon-fishings, was made by the Committee to say and unsay, to assert and contradict, those assertions, nearly in every individual passage, through an examination extending to many folio pages. When we reflect on this, it surely cannot be wondered at that the Legislature viewed the *Evidence* and *Report* as documents on which they could not place any reliance.

Notwithstanding the number of documents and mass of evidence submitted to the House of Commons by the Committee, I shall venture to offer it as my opinion that the results are below criticism. I make this statement, I hope, without going out of my way to look for materials for criticism; there are many who, viewing the question of the Salmon Fisheries as of a national and economical nature, might deem, through ignorance, the report and evidence before the House of Commons as valuable and important, and might wonder at my not having first submitted an analysis of these documents to the Society as a basis for further observation, and a still more extended inquiry: this analysis I have very carefully made twice, first, before I commenced the latter part of these inquiries and since; the conclusion come to is still the same, viz. that the persons offering the testimony and evidence here, are, without any exception, incompetent to the task; the greater part being the evidence of individuals, to whom it would be impossible even to explain the care and precision, and extent of direct evidence, requisite to arrive at a correct scientific conclusion.

APPENDIX C.

In 1830 a Natural History of the Salmon was published, by Alexander Fraser, salmon-curer and tacksman of Dochnaling, a copy of which work Sir John Sinclair had the kindness to present to me. I have thought it right thus to particularize Mr Fraser's work, not that it contains matter of any great value, but that throughout it is written with great candour and good sense. Mr F. has had the most ample opportunities of observation, as being a most extensive practical salmon-fisher. A valuable table of river-fishings is appended to the work. Mr Fraser in his preface, claims attention from the public for his work, being "the result of more than forty years almost uninterrupted practice in salmon-fishing." His experience, however, I remark, is *local*, and liable, therefore, to all the objections which lie against experience drawn from limited observation. His remarks, "that practical men who have the best, and indeed the only, opportunity of forming a correct judgment, are generally deficient in the power of expressing it, or are too actively engaged in business to trouble themselves about speculative

* The term *worm* is usually applied to earth-worm, a sort of bait which will hardly be found in the estuary of any river, and the other bait should have been characterised. A reference to Part II. will shew that gregarious fishes do not take, indiscriminately, whatever bait they can catch; and, as the question of food determines the locality of the feeding-ground, and, this being determined, settles all questions of proprietorship, it seems clear to me, that on the Committee discovering that not one of the witnesses upon the salmon-fisheries knew a single positive fact as to the food of the salmon, the most important of all the questions brought before them, and upon which every other hinged, the investigation ought to have ceased there, and not resumed until that point was fairly settled by persons competent for the task.

opinions," contain a palpable error. The true deficiency of such persons is, not that of expression, but of power of observation, arising from *two* sources: a naturally unphilosophic mind, clouded with prejudices, and, above all, a want of a proper scientific education. The observation of Mr Fraser is rooted in a feeling which is known to be very prevalent, viz. a wish to underrate the real merits of scientific men, from Newton to Davy. Those who think it worth while taking the trouble, in a question so perfectly within the reach of every thinking person, will find a triumphant refutation of such opinions as Mr Fraser's in the works of Babbage, Brewster, and others. If Watt perfected the steam-engine, every one knows, or should know, that he employed the common scientific principles of the day in doing so; and I trust that no one will have the hardihood to deny that those principles were taught to him and others of his day in a manner admitting of no competition, and in a manner probably admitting of no future improvement, by Dr Black. The path of science is too much trodden for a practical man, agreeable to Mr Fraser's acceptation of the word, to make many discoveries. What convinces me that Mr Fraser's experience is perfectly local, and consequently scarcely deserving notice, is a passage in his first section on the Habits of the Salmon, where he says, "they never *descend* lakes or rivers until near the spawning season, when they *fall down* lakes to streams and shallows." The experience of all other persons, so far as I can observe, and my own in particular, leads me to use the word *ascend*. Mr Fraser's locality, however, may be peculiar. He says, moreover, p. 6, "*clean* salmon require a worm-bait." In the Tweed, in spring, a worm-bait is successful with spent or *foul* salmon. At page 7, Mr Fraser observes (very gravely, and after indulging in the most playful fanciful notions imaginable with regard to the sight, smell, and hearing of the salmon), that "there are fifty-four joints in the salmon, which he can use quickly or slowly as he may desire." As there are evidently many hundred joints in the salmon, we must assist Mr Fraser in giving precision to his language, as to suppose that he means the vertebral column or back-bone of a salmon. To shew how difficult, nay, I should almost say impossible, it is for a person not accustomed in these matters to make a single correct observation, we will just hint to Mr Fraser, that if he will be so good as recount the individual vertebræ of a salmon, he will find that there are sixty-one (and not fifty-four), and consequently as many joints; fish having no part of the column run together, or ossified, as happens in most of the Mammalia.—Thus an experience of forty or fifty years as a salmon curer and catcher, has not enabled him to count the backbones correctly.

In respect to the food of salmon Mr Fraser has notions also perfectly local, that is, confined to himself. And as the whole passage admits of no sort of analysis, and as indeed no person having the smallest knowledge of natural objects would think it necessary to read the article twice, much less to examine it seriously, we shall simply quote his own words, and so leave it:—"Their digestion is so quick, that in a few hours not a bone is to be discovered. Of this I have had various proofs, in trouts caught by a par as bait on set lines. Fire or water could not consume them quicker. The salmon has but one intestinal canal (how many does he think a cow or a horse has) like the woodcock, which is tripled in the middle, and covered with a coat of fat, which, in a short time, dissolves every thing eaten by the fish," p. 17. No part of the present memoir,

text, or appendix, is intended to be addressed to persons whose education and habits of thought are so low as not to observe, without its being pointed out to them, the amazing errors in point of fact,—wideness of hypotheses, and abuse of what is called *non sequiturs*, in the above passage.

In speaking of insects which attack salmon, Mr Fraser has omitted making any mention of the internal parasitical animals, which we think he would have mentioned, had he seen them, or seeing them, known their nature. I confess this excites strong doubts in my mind as to the accuracy of Mr Fraser's observations generally, and causes me to undervalue altogether his forty years' experience as a salmon-curer. The reader, by referring to the history of the evidence before the House of Commons, contained in this Appendix, will there find that a practical fisherman and tacksman, who had paid thousands a year, and that for many years, to river and sea proprietors, was so totally incompetent to make any proper observations on the natural history of the salmon, that he mistook the tape-worm for the food of the salmon. Mr Fraser's remark, that he has found the Jernea upon "the gills of the breeding-salmon on the return of the latter to their rivers in the month of July or August," is peculiar to himself, and of course would require some farther confirmation. Mr Fraser's honesty and good faith induces him to put credence in the experiments regarding the return of salmon to their *own* rivers, and the growth of the smolt to the grilse within a certain time. Our remarks on this point will be found in the text; the whole is perhaps more curious than important. It seems done with the view of proving the herling an adult fish when twelve inches long, and thereby allowing the mesh of the net to be small; but the natural history of the herling is not to be come at in this manner.

As a proof of the extreme candour of Mr Fraser, I refer the reader to his chapter on Great and Small Salmon. It will be there found that Mr Fraser has a mind capable of rising above all prejudices in the pursuit of truth.

The section containing Mr Fraser's experiments on the Smolt, contains, without doubt, a number of very curious observations. When Mr Fraser has repeated them carefully, and noted a number of circumstances regarding them, which unfortunately have been omitted, I shall be most happy to notice them more particularly: for the present, I may remark, that they are very much at variance with what has been observed by Mr Buist and myself. Mr Fraser thinks he has proved that ova may remain buried to a depth of six or eight inches all summer, for at least four months, and at ten yards distant from the river.

The question of the fry getting early or late to the sea, is one which, of course, is not easily settled. Mr Fraser thinks that fry live buried in sand and pebbles from 1st April to 1st August, and probably later, and without food, and that they do not grow larger in their native beds under such circumstances, than they do when confined, as described above, in a dish of water, and he thinks it quite clear that it is these fry which are buried in gravel all summer, that "partly supply us with clean and early salmon every season." With reference to this I have just a single question to ask, and until this be fairly answered, I should deem it unnecessary to notice these opinions further. If fry thus ascend through the gravel, at various times, from the 1st April to 1st August, and probably later, how comes it that they are not taken by the angler after May, or seen in small dams, weirs, &c. by the river side?

Observations on the Natural History of the Salmon, Herring, and Vendace. By Robert Knox, F. R. S. Ed.

(*Read 7th and 21st Jan.* 1833.)

PART II.—THE HERRING AND VENDACE.

It is a fact sufficiently curious in itself, independent of all scientific consideration, that the discovery of the food of certain gregarious, important and much prized fishes, as the Herring, Vendace, Salmon, &c., should have defeated the efforts of all inquirers. Even persons the least curious, or who think at all, must be sensible that there can be nothing more calculated to excite curiosity than a fact of this kind, however indifferent they may be to zoological science. The natural history of the Common Herring alone—its importance to the nation—its incredible abundance—and the really enormous sums lavished on its encouragement by a Legislature by whom every thing nautical and commercial has uniformly been encouraged;—these considerations render a decisive step in the natural history of its food an object not so much of zoological as of commercial consequence. To the scientific part of the inquiry alone, however, it is my intention here to limit myself; the consequences and results can easily be calculated and made public at a future period and in another form.

It may be not unappropriate briefly to state the circumstances under which the inquiry into the food of the herring and of the vendace was begun. This will divest the observations of an abstract character, which in this advanced stage of the inquiry they might inadvertently assume, and give to the whole memoir that value which I trust it will be found entitled to, as well from the great extent of the inquiry itself and from its success, as from the strictly personal nature of all the observations *.

My attention was first directed to the discovery of the food and habits of the salmon, as described in the first part of this memoir ; and I should gladly have limited the inquiry to that species of fish, in consequence of the time and labour required; but I found, as all scientific men will readily understand, that the basis of observation was too narrow, and that it immediately branched out, so as to include the trout, parr, herling, vendace or corregonus, and herring. The vendace of Lochmaben being closely allied to the salmon and herring, became, therefore, the next object of inquiry, and the following is a brief account of the discovery of its food.

In certain of the romantic lakes around Lochmaben (a small town in the south of Scotland), is found the elegant and delicate vendace. A precisely similar fish has, as it seems to me, never been found in any other part of Great Britain; and even the very few persons who state its existence in Wales, and in one or two places in England, admit its extreme rarity. Its presence in the Castle Loch of Lochmaben has been traced to the times of Queen Mary, and even prior to her time. Naturalists, and more particularly Sir W. Jardine, who has given by far the best account of the Vendace, refer it to the genus *Corregonus* of systematic

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* The "Journal of Observations" on which this memoir is based, is most extensive, but it has not been thought necessary to trouble the reader with so many details.

writers, placing it, as it were, between the salmon and herring, to both of which it is in some point or other allied *.

Of this Lochmaben Vendace, which perhaps yields in delicacy as an article of food to none with which I am acquainted, it was known that they never had been taken with an angle, and that they could not be taken with any bait; it was also generally credited that they died immediately on being removed from the water, and the male fish had not been seen. I discovered the male fish, but they were few in number, or taken with more difficulty than the female, the proportion being generally one male to a dozen of females. I found also that they live as long as most fishes on being removed from the water, and that their food, previously imagined to be an unknown vegetable substance, consists exclusively of microscopic animals of the class *Entomostraca*, and which, for the apprehension of those unacquainted with natural history, we may call microscopic shell-fish, generally about $\frac{7}{12}$ths of a line in length, and abounding, as it would seem, in incredible numbers in the lakes frequented by the vendace. These entomostraca breed very frequently throughout the year, and carrying the ova about with them, as is the habit of most crustaceous and testaceous animals, they offer a rich and tempting, and at the same time most delicate, bait for the vendace; hence the delicacy of the fish, its admirable condition, want of putrescent remains in the intestines, and hence all those excellent qualities for which the *Lochmaben* vendace is so deservedly prized. It at present, in fact, is as much

* My reason for selecting the Vendace or Corregonus as the next object of inquiry, may here be stated. It was admitted to be allied to both genera, viz. to the *Clupea* and *Salmo*, and to be a fish whose food was absolutely unknown. The same was true of the Herring; but I fancied an inquiry into the food of the *Corregonus*, as being a fresh-water fish, whose *habitat* was, as it were, local or *very limited*, to be a matter more within my reach, at least compared with the herring. I imagined, also, that could I discover the food of the Corregonus, I should be led to that of the herring. The result proved me right in my conjecture.

valued by the inhabitants on the margin of the lake, as by others at a greater distance, since no ingenuity has as yet succeeded in taking them with an angle; so that they are never seen except during the annual meeting of the Vendace Club, or when fished by the special permission of Mr Murray, the proprietor, who keeps a net and boat*. It were worthy the attention of the Societies established for the encouragement and protection of British Fisheries, and of wealthy private individuals, to extend the range of the vendace, by transferring it to the numerous lakes spread over this country, its food (being now known,) being first ascertained to be present, or if not in the lake, located for a considerable period. As connected with their generation and convenient transfer to other lakes, we may remark, that the ova of the vendace were found to be very large on the 14th December, so that they evidently spawn in the depth of winter; and I have ascertained, contrary to the generally recorded opinion, that they not only bear handling, but are pretty retentive of life, after being removed from their native element, so that removal to a distance is by no means impracticable. Thus we should obtain an excellent article of food, and an addition to our markets, instead of all that tribe of fishes of the dace and bream or carp kind, which hardly any one in this country will use as food. It were easy, even in certain lakes, to procure an exclusive habitat for the vendace. The speculation were no doubt a profitable one.

The Castle Loch of Lochmaben contains twelve or thirteen different kinds of fishes, in particular all the family of the Daces of naturalists, which are so inferior as an article of food, that when taken they are given to the pigs. Trout of a very large size are said to have been taken in the lake. All the fishes in

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* I beg here to offer my best thanks to Provost Thomson, for his great liberality and kindness in allowing me to fish the lake during the summer of 1833.

the lake (the vendace excepted), are readily taken with the usual baits.

I have subjoined a drawing of the food, as seen under the excellent microscopes of Dr GREVILLE, and Mr GRAHAM DALYELL, to whom I owe my best thanks, not only for the interest they manifested in these inquiries, but likewise for the very great trouble they took in verifying my observations, and in giving me the command of their microscopes at all times. The animal, as examined in the various specimens placed under the microscope, and represented in the accompanying engraving, is either imperfect, or has not been described by authors. Its apparent imperfection may arise from its having been acted on by the gastric juices of the fish. It seems to approach nearest the Lynceus lamellatus and Trigonattus of MULLER. A few have dried up on the glass, upon which they were examined, and can still be well seen with the aid of a good microscope *.

The discovery of the food of the vendace or corregonus of Lochmaben, led immediately to that of the herring: it became impossible to overlook the strong analogy subsisting between the species; and although the one was marine and the other lacustrine, we know their differences as to habitation to be but trifling in the great economy of nature. But even here some difficulties presented themselves, although the key to the discovery was in my hands; the extremely minute microscopic shell-fish or entomostraca, which was immediately discovered to be the food of the herring, were found to be more broke down than in the case of the vendace, and the food could not in any case be got for ex-

* It is mentioned in Captain FRANKLIN's Journey to the Polar Sea, that the stomach of the Corregonus Signifer or Back's Grayling " is generally filled with gravel or black earth." This black earth, I presume, may be the remains of the testaceous entomostraca or other microscopic shell-fish, on which this kind of *Corregonus* probably subsists.

amination until it had been acted on, in all probability, for a considerable time, by the gastric juices. These difficulties were, however, at last overcome, and a tolerably perfect specimen will be found represented in the Plate. The herring then lives *as* the vendace on marine entomostraca or testaceous microscopic insects. When the herring quits the shores after spawning, it must be, as in the case of the salmon, for the purpose of repairing to the natural feeding-ground: by this it is not meant that the entomostraca do not exist on our immediate shores: the contrary of this is well known; but these cannot, for obvious reasons, be the ground in which they are found in that incredible abundance, so as to supply the wants of the herring. The extent of the supply of food may best be imagined by considering the myriads of herrings which usually visit our shores, although we should certainly form a most erroneous idea of the habits of the herring, were we to suppose that they at all times keep together in masses, as seen at the fishing-stations of the herring-fisheries of Great Britain. They (like the vendace) collect together into masses (schools) only previous to their usual spawning-season.

The circumstances over which this discovery throws light are many. It explains and establishes the striking difference observable in the herring whilst feeding on the entomostraca, and in high condition with the stomach and intestines seemingly constantly empty, and as if purposely prepared and cleansed, contrasted with the herring when out of condition, as it always is when near the spawning-season, and feeding not unfrequently on the fry of other fishes, and on that of its own species, its stomach and intestines loaded with putrescent *debris*, itself as an article of food insipid, soft, and useless. All these are facts which flow from one discovery, and which must, I think, in time, on patient inquiry, lead to important conclusions relative to a question which all must, I trust, allow to be national.

It was not indeed to be expected, that, in the total absence

of positive inquiry and of fixed results, any legislative enactments were likely to prove beneficial, or ought ever with propriety to have been attempted. Pennant, our illustrious British naturalist, but a few years ago traced the progress of the vast shoals of herring from the Northern Ocean and from the Poles to the British shores, evidently labouring under the erroneous impression that they always kept together in masses; and this statement, which at the time most persons assumed to be a fact, is now declared by the compilers of works on Natural History to be a mere fable and romance. From this great and leading fact, the discovery of the proper food of the herring and corregonus, flow considerations of a nature which it would be difficult at the present moment fully to state. The extremely erroneous notions of its habits and *habitat* become easily corrected, and its state both previous to and after visiting our shores fully appreciated.

But a full inquiry into the natural and economical history of the herring, is, I fear, beyond the reach of any one whose time is necessarily so fully occupied as mine: it would also be a troublesome and expensive inquiry, and would lead me away from my proper business and pursuits into a field of boundless national utility no doubt, but devoid of all personal interest or advantage.

EXPLANATION OF PLATE XI.

Fig. I. Food of the *clean* Salmon. The *Asterias glacialis*, or Cross-fish, here represented, is the most common of its kind. The dorsal integumentary surface has been cut away, together with all the internal soft organs, except the ova, and consequently the view given is that of the skeleton of the internal abdominal surface. The animal has uniformly five rays, one of which only is fully represented. The ova (*c*), inclosed by an extremely delicate oviduct, have been drawn out, in order to display their appearance more completely. In the living animal, they occupy the grooves on each side the mesial line of each ray, and are of course entirely concealed until a section is made.

a, The finished ray.

b, The central opening of the animal.

c, The ova, which, in the recent state, when fully developed, are of a rich cream colour. They form (with the eggs of other Actinodermata and Crustacea) the food of the salmon.

Note.—In these animals, the ova, during their progress to complete development, pass through a great variety of colours, but, when fully formed, assume invariably the distinguishing shade observed in the adult animal. In the Kelt, for instance, a species having thirteen rays, the ova are of a bright clear vermilion.

Fig. II. Food of the Herring, taken off the Isle of May during the month of December 1832.

A, Natural size of the adult full-grown animal.

B, Magnified view of the most perfect specimen which could be found.

C, Magnified view of a detached spine, being appendages of the legs and feelers of the adult animal: they, together with the testaceous cases covering the body of the animal, form the brownish layer found in the stomach of the *clean* herring, as mentioned in the text. Thousands of them were raised on the point of a delicate silver probe.

Fig. III. Food of the Vendace, taken in the Castle Loch of Lochmaben 18th September 1832.

A, Natural size of the animal.

B, A side view magnified.

a, The antennæ or feelers.

b, The eye.

FIG. IV. Food of the Vendace, from the same locality, taken 14th December 1832.

A, Natural size of the animal.

B, View from behind, magnified.

a, The antennæ or feelers.

b, The eye.

c, Organs of a reddish colour, supposed to be the ova.

FIG. V. Food of the Vendace, from the same locality, taken 7th September 1833.

A, Anterior segment of the animal, magnified, and viewed from behind. This portion is commonly found in the stomach of the Vendace, detached from the body of the animal.

B, Side view of the adult animal, magnified. The most perfect specimen hitherto observed.

a, The antennæ or feelers.

b, The eye.

c, Organs of a rich orange colour, supposed to be the ova.

The following preparations were exhibited during the reading of the papers to the Royal Society, and will be found in my Museum:

No. 2. Ova of the Salmon, taken from the spawning-bed in a tributary stream of the Tweed 31st March 1832.

3. Ditto, from ditto, 2d April 1832.

4. Ditto, from ditto, 16th April 1832.

5. Smoult (average size of five or six dozen), taken from the same stream 22d April 1833.

7. Viscera of a Smoult, exhibiting the development which has already taken place in the ova.

8. Specimens of the Caligus productus and Caligus curtus, from Herlings taken in stake-nets near Annan.

9. Skeleton of a Grilse.

10. Food of the Salmon whilst in the sea.

11. Skeleton of the Asteria glacialis.

12. Skeleton of the Asteria papposa.

13. Food of the herring from the deep sea.

14. Viscera of the Herring *when in prime condition*.

15. Skeleton of the Herring.

16. Food of the Vendace.

17. Viscera of the Vendace at all seasons.

18. Skeleton of the Vendace.

APPENDIX TO PART II.

Opinions of Authors, &c.

I have ever observed that real practical fishermen, those whose experience is most extensive, have uniformly stated the nature of the food of the herring to be most mysterious, and have denied, with the pertinacity excusable to truth, that any food recognisable by ordinary sight, can be detected in the stomach of the herring, if the fish be at all in tolerable condition. They knew that occasionally, and in a few, the fry of other fishes, and even of the herring, might be found in their stomachs; but this, with much judgment, they considered as in no shape connected with the actual nature of the usual proper food of the herring, whilst in the deep seas, or even on our coasts. The preceding discoveries shewing, that by the microscope alone (an instrument of course never used by the practical fisherman), that peculiar food on which all the good qualities of the herring seem to depend, and which they seek for at a distance from our shores, confirms the judgment of the practical fisherman and unbiassed observer, unconnected with trade, against the coarse and ill digested remarks of the half educated, who, misinformed almost on every point, commence by *supposing* that herrings must live on fishes smaller than themselves, young crabs, lobsters, and in short, whatever they can catch, forgetting, at the same time, and not being able to understand, *that the discovery of the food of the vendace (corregonus), decides the question as to the food of its allied genus the Herring;* and on the real food being discovered and pointed out, together with the erroneous nature of their statements, finish by asserting, with amazing effrontery, that *the food* of the herring had been known to all scientific men for centuries!

The deplorable obstinacy and obliquity of reasoning which lead to an assertion of this kind, become manifest by considering the following very brief account of the most recent inquiries into the food of the herring, as contained in a work of easy access and of the highest value—the Transactions of the Highland Society of Scotland. The extracts, I am aware, will neither silence the pseudo-philanthropist nor the *popularly educated* man; but it will convince any unprejudiced person, that assertions like those above alluded to, are without the slightest foundation.

The reply of the practical fisherman to the ill founded assertion of the half educated, who pretend an acquaintance with the natural food of the herring, vendace, salmon, &c. was clearly expressed, and nearly in the following terms:—

Why is it, say they, that thousands of prime salmon have been opened and the stomach and intestines found invariably empty? Why is it that, if the herring be in good condition and fit for the food of man, the stomach and intestines present the same appearance as in the salmon,—that is, empty apparently of food, with clear and semitransparent tunics, free from all symptoms of putrescence and of coarse intestinal products, as if they had purposely been so prepared? Now the fishermen not knowing what to

make of all this, assert boldly that these fish live by *suction;* unaccustomed to attach a precise meaning to his language, this word *suction* comes in the place of an idea, though it be in this particular case a phrase altogether unintelligible; but if they really attach any meaning to the word, they mean that the herring lives after the mode of plants. Modern systematic works on natural history, so far as I have been able to observe, maintain a profound silence as to the food of the herring, and indeed their observations generally shew, that no dependence whatever can be placed on any published account of the natural history of this most important fish. In such works, all mention of the food is either omitted, or, what is much worse, mistaken, and consequently their habits, and I may venture to say also their *habitat* or place of residence in the ocean.

The Rev. Dr WALKER observes, "that he had examined the stomach of herrings at different seasons of the year, without finding in it any sort of palpable aliment." P. 274. "On their first appearance off the Lewis in the month of July, when they were full grown and very fat, nothing appeared in their stomach but a little slime." P. 275.

"During the residence of the herrings on the coast of Scotland, we know of no food they use, and it is probable they require little or none, except some attenuated alimentary matter which the sea-water may afford them." P. 275.

"We think it not altogether improbable that they may live on a small species of medusa, or some similar marine animal, which is not as yet known to naturalists." P. 276 *.

Mr J. MACKENZIE asserts that "it has been frequently observed that the small fry suck their nutrition out of the marine algæ, or from some matter adhering to them." P. 313. But it is almost superfluous to remark, nothing of the kind has since been observed by any one, so far as is recorded.

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* Contrast these cautious observations of this strictly correct, scientific, and candid person, and the opinions of the gentlemen who follow, with the following "opinions" of persons misnamed practical men, and who, having sold herrings, and at one period, perhaps, attended a course of lectures on natural history, fancy themselves competent to speak and write about zoological matters. A person of this kind (to whom I have replied elsewhere) has made the following extraordinary statements and discoveries:—1st, That "the Bounty System placed the Scottish herrings in direct and successful competition with the herrings cured by the Dutch in most of the markets of the Continent." My reply is, that the whole of the exports of herrings from Britain to the Continent of Europe in 1830, amounted to 24,960 barrels, and that the Dutch trade to Germany alone *used* to be 130,000.

2dly, He asserts that "the Dutch fish close to our own shores." My answer is, that I know the contrary.

3dly, He states that the inferiority in "the Scottish herrings at some localities is owing to their not being bled or gutted immediately when taken out of the sea." My answer is, that the Lochfine herrings are the best in Scotland, and they are not gutted at all, so far as I have observed; the gills and heart merely seem to be removed.

4thly, He affirms that "the entomostraca are *young crabs* and *lobsters;*" which is just as if we were to say, that sheep are young oxen, not yet grown.

5thly, He has misquoted the Report of the Herring Fisheries, and finally asserted, that "the food of the herring, as I have described it, was known to scientific men for hundreds of years." In 1833, Professor RENNIE, of the King's College, London, declares the food of the herring to be altogether unknown; but a perusal of the quotations from the Transactions of the Highland Society will convince my readers that I could not trust myself with a reply suited to such an assertion.

The compiler of the article "Herring Fisheries," in the Supplement to the Encyc. Brit. p. 257, gives a very amusing account of the habits and food of the herring; unfortunately though very romantic, it is an error from beginning to end. "It has been generally thought," observes this ingenious writer, whose name I do not know, "that their winter habitation is within the arctic circle, under those vast fields of ice which cover the northern ocean, where it fattens on the swarms of shrimps and other marine insects which abound in those seas, and which afford also the principal food of the whale. Here it is supposed they deposit their spawn, and on the return of the sun towards the northern hemisphere, again rush forth in those multitudinous hosts which exceed the power of the imagination to conceive."

Fabricius, in his Fauna Grœnlandica, 1780, p. 182, says of the "Clupea Harengis," "De cibo ejus nil certi indicare queo dum nunquam escam in ventriculo ejus inveni."

"On what they live (observes Mr Drummond, p. 7. vol. iii.) we can only form a conjecture. I have seen their stomachs opened at all seasons of the year in which they appear here, but never found any thing in them, excepting some slimy matter. The same peculiarity is to be found in the whole genus: it is to be observed in many fishes, that while the milts and roes are in an increasing state, they are very careless of food. The shad, the herring, the pilchard, the smelt, come to us all fat, and daily decline until they leave us when shotten. A friend of mine, when at Loch Lhynn, near Fort William, this season, upon the arrival of the boats, cut up many herrings, but never discovered any thing in their stomachs: the fishermen, however, assured him that they frequently got in the foul, or spent, fish, several of their own fry, sand-eels, &c. It has been asserted, that, to the northward of Shetland, they feed and fatten on a species of medusa which is to be seen there in great abundance. I cannot help combating this opinion, as I have observed various blubbers, especially that kind called by sailors the Portuguese Man of War floating in the Atlantic, where different fishes swarm around it and never offered to touch it. I never took any of these blubbers in my hand without having it disagreeably blistered; I naturally conclude that they cannot be a delicate morsel for the herring."

Mr Drummond was the first, or among the first, to question the views of Mr Pennant regarding the migration of the herring, denying, on probable grounds, that the range of its migration was so extensive as that distinguished naturalist imagined, and viewing the notions of its proceeding at each migration towards the icy seas of the Arctic circle as a mere fable. On these grounds, and on these alone, I think he advocated the deep sea fishery; what I mean is, he asserted that fish found in deep water are acknowledged to be superior in quality to those caught near the shores; and that the deep sea fishing would prove a valuable nursery for seamen: with the latter question I have nothing to do in this memoir; the former opinion is also mine, but on grounds entirely different from those advocated by Mr Drummond.

The reason why the food could not be discovered by preceding observers will be readily understood by most of my readers; it was next to an impossibility for any other than a scientific person, who had examined the whole range of the animal kingdom, to make out the inquiry; for, first, we have seen that it requires a certain degree of anatomical knowledge to examine the stomach and intestines, and considerable dexterity

in the use of microscopes of different focuses; secondly, extended zoological knowledge to avoid running into errors involving amazing absurdities, such as that of the practical salmon-fisher, who had, or at least *said* he had, examined thousands and thousands of salmon, and yet was so ignorant as to mistake the tape-worm for the food of the salmon!! And, lastly, in the present instance, the possibility of mistaking the entomostraca present in the stomach of the herring for animals whose presence there was accidental. I am aware that there are many, whose regard for accuracy in scientific statements being extremely coarse and loose, will not only assert that they had examined the stomach of the herring, but had also seen its food. The sight of these persons must have been amazingly acute, it being *certain that they did not use a microscope! according to their account they have seen with the naked sight* objects which became visible to others only by means of glasses of 120 powers! But this is not all. Having seen objects without microscopes which can only be seen through their aid, they, in the next passage, assert that nothing of this kind exists, and mingle up their crude notions of scientific matters with the strongly biassed notions drawn from trade. The result has been particularly described by Mr Babbage in his admirable work on Manufactures, to which I beg leave to refer the reader.

Herrings are stated to have first appeared in considerable numbers in the Firth of Forth about 1794. The herring-fishing managed by boats from Berwick, but a considerable way to the south of that port, appeared, in 1832, to be very successful, and was conducted with great activity.

Spawning Season.—The herrings brought to Edinburgh in the beginning of August in 1833, and said to come from Dunbar, ought not to have appeared in any market; they were, without doubt, the first which had been taken off that coast; they were, in the mean time, actually spawning, many of them had already spawned, so that the taking them was, to say the least of it, extremely improper *.

The herring taken on the west coast of Scotland, in and about the lakes, are, so far as I can learn, and in some true Lochfine salted herring, I have personally observed, in a totally different state from those taken on the east. The feeding ground is evidently at a considerable distance from the east coast; whereas, on the west, it is by no means improbable that the food of the herring may be found, either in the lakes or at no great distance. Certain it is, that these herrings are very generally in prime condition, more particularly when procured on the spot; and when taken at the proper time, are in every respect unlike those caught on our eastern shore; on the latter locality they are, when taken, nearly ready for spawning at the moment of their capture; on the west coast the organs of generation are but little developed.

* Since reading my paper to the Society, I observe some attempts are making to improve, or vary the mode of preparing, the herring for the market. I observe some offered for sale kippered, with the whole viscera and organ of generation, &c. entirely removed; but these herring are not good food, *not being taken at the proper time and place!* It is a vile trick of trade, worthy of its inventor; and of precisely the same nature as the kippering of salmon during the months of October and November.

In the stomach of the herring, when in good condition (and this may be readily known from their external appearance), the remains of hundreds of minute Entomostraca may be found, *and nothing else*. All the internal organs seem purified and semitransparent, the stomach contracted, and, though examined with the ordinary botanical microscope of naturalists, appear empty, the inner surface being covered only with a thin coating of the usual mucous substance, and spread over this a layer of a brownish coloured substance; the brown substance is composed of the testaceons cases, antennæ, and feet of animals, none of which animals altogether, when entire, exceeding the 7–12ths *of a line*. When the herring takes to other food, as the fry of herring, &c. and which I suspect it *does only after being some time on the coast*, the stomach becomes distended, as it were; the whole of the internal organs, although examined within three hours of being taken, are soft and dirty; they run almost instantly into putrescence, and in the very best prepared herring, either as red or salted, the milt and roe, which is generally left, I observe to have become more than half putrid; and the herring itself, as an article of food, is really good for nothing, and in truth is only used as food by the poor. The fishermen themselves have an obscure notion that the herring is taken in different states, and they say that those taken at the commencement of the season, at Caithness, &c. will scarcely salt, owing to their extreme richness. It is probable that these herring, if carefully prepared and pickled, would bring a very high price. Persons accustomed to eat Lochfine and Lochlong herrings will not eat any other kind; and I will now predict that the quantity of herring taken there, and which may have taken to the fry of their own species, will be found in an inverse ratio to those taken in the Firth of Forth, although the entomostraca is to be got in or near the Firth of Forth, but in all probability in less abundance; or it may become scarce, from the quantity of herring congregated into a narrow space, the herring having approached the shores of the Forth for the special purpose of spawning.

The peculiar species of entomostraca which I have detected in the herring of our coast, is difficult to determine; they are invariably very much broke up. I consider them, however, to approach very nearly the cyclops of M. Dumeril; and it is sufficiently striking to observe, that the vendace is feeding in the fresh water lake of Lochmaben on a similar animal. Muller also, in his work on the Entomostraca seu Insecta Testacea, gives a drawing of an animal to which the detached portions found in the stomach of the herring bear a strong resemblance. He describes it in the following terms:—"Cyclops longicornis, antennis linearibus longissimus cauda bifida;" and to this he adds the following remarkable observation, which, if followed out, or rather preceded by extensive scientific research, might have led him to a discovery of the real food of the herring. "In mari Timmarchia alluenti reperit immortalis Gunner, ego postea in sinu Drobactiorum ac in ventriculo clypea conglomeratos absque instituto examine vidi."

Muller considered the rare entomostraca as being mostly the prey of Hydræ, &c.; and, from their abundance in fresh and salt water, he constantly reminds us that water drinkers must be content often to swallow thousands of these animals at a draught.

In the stomachs of the vendace sent me 14th December last (1832), I observe the little animal of which this fish devours immense quantities, is not the same as the one he lived exclusively on during the autumnal months. I subjoin a sketch of that found 14th

December, and specimens can be placed before the members of the Society, at any time, in a soft state.

One of the results of these inquiries I imagine to give an importance to the discoveries of Mr Robert Brown, which they did not seem previously entitled to; and likewise I admit freely, an importance to microscopic research against which I was myself a few years ago very unjustly and perhaps unfortunately prejudiced.

In all the herring I have examined in the course of this inquiry, I have found male and female, the number greatly preponderating in favour of the female, in as much as six to one. The largest herring I have met with measured eleven inches; the largest male herring I have met with measured nine and a half inches. The same is the case with the vendace of Lochmaben. The proportion in favour of females is so great, that I was the first to shew the male, and I have not found any male to exceed 7 inches, whereas the female are often 8½ or 9 inches long. The organs of generation were, in the greater number large; in a few, however, they seemed at the *minimum*. In these, (*i. e.* with the organ of generation not increased), the mesentery was uniformly loaded with fat; the stomach always contained the proper food; and the fish altogether in such a state as seemed to me to require little or no cleaning before being used as food. The spleen was generally remarkably large and fleshy. This organ seems even to be of different forms in different specimens of the herring.

Dutch Herrings and Mode of Cure.—The Dutch take and cure bad herrings as well as good ones. This they must of necessity do when the busses approach very near to the spawning ground, which, however, is seldom the case. Their selection, however, is very carefully made, or at least it used to be so. Holland has changed in many respects since the prosperous days of "The States."

It is very questionable if, upon the eastern shores of Scotland, any considerable take of perfectly clean and prime conditioned herrings, and which are neither full nor shotten, ever takes place. This kind of fishing seems to me to bear the same relation to the deep-sea fisheries of the Dutch, which the salmon-fishing of Peebles bears to the stake-net fisheries of the Bay of Berwick. It is easy for persons in trade, as has been done, to assert that salmon and herrings are equally good wherever they are taken, for with such persons the article they have to dispose of is always the best; and, having an interest in asserting it, they believe in the assertion, and thus believing they will swear to it; but nothing of all this has any thing to do with scientific truths.

Trade.—One great and manifest error in the conducting the British Herring Fisheries has been the endeavouring to make that appear to be what, in point of fact, it was not. Thus the writer of the article Fisheries, in the Encyclopædia Britannica, says, "In the Report of the Downs Society of Fishermen, it is stated, that herrings had been taken within the Cinque Ports, of a quality so nearly resembling the deep sea fish that they were cured and sold as the best Dutch herrings." P. 265. The effects of this kind of conduct on British trade and British manufacture, has been shewn by Sir Charles Babbage to be most deplorable. "We brought with us from England nearly 100,000 needles of various sizes, and amongst them was a great quantity of 'Whitechapel

sharps,' warranted superfine, and not to cut in the eye! Thus highly recommended, we imagined that these needles must have been excellent indeed; but what was our surprise some time ago, when a number of them which we had disposed of, was returned to us with a complaint that they were all *eyeless*, &c., so that to save our credit we have been obliged to throw them away." Landers' Travels to the Niger, p. 43. From this moment the Landers found *English needles unsaleable*. So much for trade.

CORRIGENDA.

Page 33. line 3. from bottom, *for* Hallbeath, *read* Halleath,
Page 52. line 13. from top, *for* Actinodermata *read* Echinodermata

Observations on the Lines of the Solar Spectrum, and on those produced by the Earth's Atmosphere, and by the action of Nitrous Acid Gas. By Sir DAVID BREWSTER, K. H. LL. D. F.R.S. LOND. V. P. R. S. EDIN.

(Read 15th April 1833).

IN a paper on the Monochromatic Lamp, &c., read before this Society on the 15th April 1822, and published in their Transactions, I recorded some of my earliest experiments on the action of coloured media on the Solar Spectrum. These experiments were continued at irregular intervals, with the view of obtaining distinguishing characters of coloured media, of investigating the cause of the colours of natural bodies, and of examining more correctly the phenomena of the overlapping colours of equal refrangibility, the discovery of which I had announced in the paper already referred to. The results to which I was conducted on the two last of these subjects, have been already communicated to the Society in two papers, one on the Analysis of Solar light, and the other on the Colours of Natural Bodies.

The first and the principal object of my inquiries, namely, the discovery of a general principle of chemical analysis, in which simple and compound bodies might be characterised by their action on definite parts of the spectrum, still remained to be investigated. The coloured juices of plants—artificial salts and their solutions, and various glasses and minerals—had afforded me many beautiful examples of this species of action, and after determining the locality of these actions in reference to FRAUNHOFER'S

principal lines, and their intensity as depending on the thickness of the absorbing medium, and the brightness of the spectrum; I was able to distinguish all such compounds, by merely looking through them at a well formed spectrum. Even in those cases where the eye could recognise no difference between the colours of two substances that exercised different specific actions upon light, their discrimination was instantly effected by viewing them through a standard coloured medium.

As some of these bodies attacked the spectrum at *two*, *three*, *four*, and even *five* or more points at once, it became probable that the number and intensity of such actions depended on the number and nature of the elements which entered into the composition of the body, or, what is nearly the same thing, that it was the sum of all the separate actions of such elements; and hence the next step in the inquiry was, to determine the action of elementary bodies on the solar spectrum. This inquiry was not limited to coloured bodies, for it is quite possible that a body may transmit light perfectly white, and yet exercise a definite action in absorbing various parts of the spectrum. The only physical condition which is necessary in this case is, that the sum of all the rays thus absorbed shall constitute white light.

The first substances which I examined were *Sulphur* and *Iodine* vapour. The *Sulphur* attacked the *violet* end of the spectrum with great force, and, when combined with arsenic, in the form of native orpiment, its absorptive power for the same colours was greatly increased. Even with the thinnest film that I could detach, and not exceeding the two-hundredth part of an inch, the spectrum was, as it were, cut sharply in two near the boundary of the *Green* and *Indigo* spaces, and this body possessed the very uncommon property of having nearly the same colour at small as at great thicknesses. By increasing the thickness, the absorption advances almost imperceptibly from the remaining blue border, and if the transparency continued, the transmitted light would certainly become *red* at great thicknesses,—a property which may

be communicated transiently to the thinnest plates, merely by an increase of temperature.

The Iodine vapour acted powerfully upon the middle of the spectrum, and, by an increase of thickness, gradually extended its absorption towards both extremities; but more rapidly towards the violet one, so as to shew that the final colour must be a homogeneous red.

In so far as these two experiments went, they were highly favourable to the speculation which had at first presented itself to me. My attention was now directed to the action of gaseous bodies, and the first trial which I made was with *Nitrous Acid Gas*. The result of this experiment completely destroyed the hypothesis which had appeared so plausible, and presented me with a phenomenon so extraordinary in its aspect,—bearing so strongly on the rival theories of light,—extending so widely the resources of the practical optician, and lying so close to the root of atomical science, that I am persuaded it will open up a field of research, which will exhaust the labours of philosophers for centuries to come.

The spectrum of Newton, and of all the philosophers of the 18th century, was a parallelogram of light, with circular ends, in which the *seven* colours gradually shaded into each other without any interruption. The illumination was a maximum in the yellow rays, and the light decayed by insensible degrees towards the red and the violet extremities. In the year 1808, Dr Wollaston conceived the happy idea of examining a beam of light, that passed through an aperture only the twentieth of an inch wide, and he was surprised to see it crossed by seven dark lines, perpendicular to its length.

About ten or twelve years afterwards, the celebrated optician Joseph Fraunhofer, without knowing what had been done by Dr Wollaston, observed the spectrum formed by the sun's light transmitted through small apertures; and by applying a telescope behind the prism, he discovered about 600 parallel dark lines

traversing the spectrum. As no such lines appeared in the spectra of white flames, Fraunhofer considered them as having their origin in the nature of the light of the sun. The strongest of these lines were seen in the spectra of the Moon, Mars, and Venus, and, by means of very fine instruments, he was able to detect one or two of them with other new lines in the spectra of Sirius and Castor.

Such was the state of the subject, when I made the experiment already referred to on nitrous acid gas. Upon examining with a fine prism of rock-salt, with the largest possible refracting angle (nearly 78°), the light of a lamp transmitted through a small thickness of the gas, whose colour was a very pale straw yellow, I was surprised to observe the spectrum crossed with hundreds of lines or bands, far more distinct than those of the solar spectrum. The lines were sharpest and darkest in the violet and blue spaces, fainter in the green, and extremely faint in the yellow and red spaces. Upon increasing, however, the thickness of the gas, the lines grew more and more distinct in the yellow and red spaces, and became broader in the blue and violet, a general absorption advancing from the violet extremity, while a specific absorption was advancing on each side of the fixed lines in the spectrum. It was not easy to obtain a sufficient thickness of gas to develope the lines at the red extremity, but I found that heat produced the same absorptive power as increase of thickness, and, by bringing a tube containing a thickness of half an inch of gas to a high temperature, I was able to render every line and band in the red rays distinctly visible.

The power of heat alone to render a gas, which is almost colourless, as red as blood without decomposing it, is in itself a most singular result; and my surprise was greatly increased when I afterwards succeeded in rendering the same pale nitrous acid gas so absolutely black by heat, that not a ray of the brightest summer's sun was capable of penetrating it. In making this experiment, the tubes frequently exploded, but, by using a mask

of mica, and thick gloves, and placing the tubes in cylinders of tinned iron with narrow slits to admit the light, there is little danger of any serious accident.

When the gas is in the liquid state, it produces none of the fixed lines which I have described, and exercises no other action upon the spectrum than any ordinary fluid of the same orange colour.

In examining the structure of the solar spectrum, FRAUNHOFER seems to have put forth all his strength in determining the position of the principal lines, A, B, C, D, E, *b*, F, G and H,* which he had selected as equidistant as possible, for the purpose of measuring their angular distances in different media, and thus obtaining the most accurate data for the construction of the achromatic telescope. These measures he has given with the greatest exactness for various kinds of crown and flint glass and for a few fluids, and he has thus put it in the power of the practical optician to construct achromatic object-glasses, with a degree of certainty and perfection hitherto unknown.

This method, however, notwithstanding its high value, is not easily applicable in practice, and from the nice observations which it involves, we have reason to believe that it has not been used by any other artist than FRAUNHOFER himself. The difficulty of procuring out of the mass of glass to be employed, prisms sufficiently pure to show such narrow lines as E, or the two which constitute D †,—of obtaining the sun when his light is wanted, and of observing and measuring the distances of the fixed lines in a spectrum constantly in motion, are insurmountable obstacles to the general adoption of so refined a method of measuring dispersive powers.

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* Six of these, viz. B, D, *b*, F, G, and H, were discovered by Dr WOLLASTON.

† These lines are also the most important, as the most luminous part of the spectrum lies between them.

From all these difficulties, the discovery of lines in the nitrous acid gas spectrum completely relieves us. As the lines whose distances are required, may be made as broad and black as we please, prisms of ordinary purity are sufficient to exhibit them in perfect distinctness. The artificial light of a lamp can be commanded at any hour, and as its rays are absolutely fixed, the least experienced observer can have no difficulty in measuring the distances of the fixed lines, and thus obtaining, with extreme accuracy, all the data for the construction of achromatic instruments.

But it is not merely to this practical purpose that the gaseous lines are singularly applicable. Among the various solids and fluids in nature, there are very few sufficiently pure and transparent, to enable us to see through them the lines of the solar spectrum, so as to enable us to measure their refractive and dispersive powers with minute accuracy, whereas the gaseous lines can be rendered visible, however imperfectly the spectrum may be formed. In determining the various elements of double refraction and polarisation, and in all optical researches where the phenomena vary with the refrangibility of the rays, the gaseous lines will hereafter perform a most important part.

Had the solar lines been much broader than they are, we might have been able, by means of minute thermometers, to have ascertained the temperature of all those parts of the spectrum where there was no light, and thus to have determined whether or not the rays of light and heat are separate and independent emanations. The phenomena of the nitrous acid gas spectrum, the lines of which can be widened at pleasure, enable us to perform this and other interesting experiments, and thus to decide many important questions in the theory of radiant matter.

From the various experiments which I had made on the absorptive action of coloured media, I was led to a general principle, which, in that stage of the inquiry, appeared to possess considerable importance. The points of maximum absorption exhi-

bited a distinct coincidence with some of the principal dark lines in the solar spectrum, and thus indicated that these lines marked, as it were, weak points of the spectrum, on which the elements of material bodies, whether they existed in the solar atmosphere or in coloured solids and fluids, exercised a particular influence. These actions, however, were so indefinite, that, with the exception of the oxalate of chromium and potash, a salt of most remarkable properties, they never appeared in the form of lines or distinct bands. The light which was left shaded into the dark spaces, and therefore, notwithstanding the general coincidence which I had observed, the phenomena of ordinary absorption could not be identified with those of the definite actions by which the solar lines are produced.

This point of similarity, however, led me to institute a diligent comparison between the solar lines and those of the nitrous acid gas spectrum; and it did not require many experiments to prove, that there existed between these two classes of phenomena a most remarkable coincidence. In order to afford ocular demonstration of this fact, I formed the solar and the gaseous spectrum with light passing through the same aperture, so that the lines in the one stood opposite those on the other, like the divisions in the vernier and the limb of a circle, and their coincidence or non-coincidence became a matter of simple observation. I then superimposed the two spectra, when they were both formed by solar light, and thus exhibited at once the two series of lines, with all their coincidences, and all their apparent deviations from it. Professor Airy, to whom I shewed this experiment, remarked, that he saw the one set of lines through the other, which is an accurate description of a phenomenon, perhaps one of the most splendid in physical optics, whether we consider it as appealing to the eye or to the judgment.

The general coincidence, thus cognizable by the eye, requires to be more particularly explained. Though some of the larger lines in the gaseous spectrum coincide with some of the larger

ones in the solar spectrum, yet, in many cases, faint and narrow lines in the one coincided with strong and broad lines in the other; and there were some strong gaseous lines, and even broad bands, to which I could discover no counterpart in FRAUNHOFER'S map of the spectrum, which, at this stage of my inquiry, was the standard to which I appealed. This discrepancy at first embarrassed me, and, as I observed it in parts of the spectrum where FRAUNHOFER had laid down every line which he had seen with his finest instruments, I abandoned all hopes of being able to establish the general principle of their identity. I was therefore obliged either to renounce this principle as one contradicted, or rather not confirmed by observation, or to consider FRAUNHOFER'S delineation as in fault, and to enter upon the Herculean task of making a better map of the spectrum.

The magnificence of FRAUNHOFER'S instruments,—the means of nice observation which he had at his command,—and his great skill as an observer, were considerations which long deterred me from even attempting to repeat his examination of the spectrum. Possessing such inferior means, and situated in so unfavourable a climate, I should have felt the attempt as presumptuous; but in the comparison which I had already made of the gaseous and solar lines, I had detected grave errors, and inexplicable omissions, in FRAUNHOFER'S map, and was disposed even to adopt the suggestion of Mr H. F. TALBOT, (to whom I mentioned the fact, and who had the same confidence that I had in FRAUNHOFER'S accuracy) that a change might have taken place in the light of the sun itself, and that the delineation of the Bavarian philosopher might have been perfectly accurate at the time when it was executed. This supposition, however, became less and less tenable as I proceeded in the identification of the two classes of lines; but even if it had been otherwise, it would have added a still more powerful motive, while it afforded the best apology for undertaking a new delineation of the spectrum.

The apparatus which I had at my command for this investi-

gation were two very fine rock-salt prisms, executed by myself; a large hollow prism made of plates of parallel glass for holding fluids; a fine plate glass prism, executed by FRAUNHOFER, and which I owe to the kindness of Mr TALBOT; a copious supply of oil of cassia and oil of cinnamon, which Mr GEORGE SWINTON transmitted to me from Bengal with his usual liberality; a good achromatic telescope, by BERGE; and an excellent wire micrometer by TROUGHTON. To this apparatus Mr ROBISON made two important additions, which he executed with his own hands, the one a brass stand with a variable aperture for admitting the incident light, and the other a stage for holding and adjusting the prisms in front of the object glass; and I have recently been favoured, by Sir JAMES SOUTH, with the use of his fine five-feet achromatic telescope, executed by DOLLOND.

After a little practice in the observation of the solar spectrum, I discovered most of the lines, which I had in vain sought for in FRAUNHOFER's map, as the counterpart of those in the gaseous spectrum. I saw well marked groups, of which he had only given one of the lines, and shaded bands, and well defined lines, which his methods of observation had not permitted him to discover. After I had laid down all the principal features on the spectrum, I was able to examine the two classes of lines *pari passu*. The action of the gas upon invisible lines in the spectrum rendered them visible by slightly enlarging them, and this enlargement of a solar line indicated the existence of a corresponding line in the gaseous spectrum.

By this double process, and by methods of observation which I believe have never before been used in optical researches, I have been able to execute three different maps of the spectrum; *first*, a map of the lines in the solar spectrum; *secondly*, a map of the same spectrum, exhibiting at the same time the action of nitrous acid gas upon solar light, previously deprived of a number of its definite rays; and, *thirdly*, a map shewing the action of the gas

upon a continuous and uninterrupted spectrum of artificial white light. The general scale of these delineations is *four* times greater than that of Fraunhofer, but some portions of them are drawn on a scale *twelve* times greater, which became necessary from the impossibility of representing in narrower limits the numerous lines and bands which I have discovered. The length of Fraunhofer's spectrum is $15\frac{1}{2}$ inches. Mine, upon the same scale, is nearly 17 inches. The length of the general spectrum, which I have delineated, is about *five* feet 8 inches, and the length of a spectrum, corresponding to the scale on which I have delineated parts of it, is *seventeen* feet.

Fraunhofer has laid down in his map 354 lines, but in the delineations which I have executed, the spectrum is divided into more than 2000 visible and easily recognised portions, separated from each other by lines more or less marked, according as we use the simple solar spectrum, or the solar and gaseous spectrum combined, or the gaseous spectrum itself, in which any breadth can be given to the dark spaces.

The suggestion of Mr Talbot induced me to watch narrowly the state of the defective solar lines at different seasons of the year, in order to observe if any change took place in the combustion by which the sun's light is generated, or in the solar atmosphere through which it must pass. Such changes I have found to be very general in every species of terrestrial flame. The definite yellow rays which exist in almost all white lights, flicker with a variable lustre; and analogous rays in the green and blue spaces proceeding from the bottom of the flame, exhibit the same inconstancy of illumination. In the course of the winter observations, I observed distinct lines and bands in the *red* and *green* spaces, which at other times wholly disappeared; but a diligent comparison of these observations soon shewed, that these *lines and bands depended on the proximity of the sun to the horizon, and were produced by the absorptive action of the earth's atmosphere.* I have no hesitation, therefore, in affirming, that, during the

period of my own observations, no change has taken place either in the dark lines or luminous bands of the solar spectrum; a result which seems to indicate, that the apparent body of the sun is not a flame in the ordinary sense of the word, but a solid body or coating raised by intense heat to a state of brilliant incandescence.

The atmospheric lines, as they may be called, or those lines and bands which are absorbed by the elements of our atmosphere, have their distinctness a maximum, when the sun sinks beneath the horizon. The study of them, consequently, becomes exceedingly difficult in a climate where this luminary, even in a serene day, almost always sets in clouds; but as I have availed myself of every favourable moment for observation, I have been able to execute a tolerably accurate delineation of the atmospheric spectrum.

It is a curious circumstance, that the atmosphere acts very powerfully round the line D, and on the space immediately on the least refrangible side of it. It developes a beautiful line in the middle of the double line D, and by enlarging a group of small lines on the red side of D, it creates a band almost as dark as the triple line D itself. It widens generally all the lines, but especially the darkest one which I call *m* between C and D. It developes a band on the least refrangible side of *m*, and it acts especially upon several lines, and developes a separate band on the most refrangible side of C. The lines A, B, and C are greatly widened, and lines and bands are particularly developed between A and B, and generally throughout all the red space.

Most of the lines thus widened by the atmosphere are faint lines previously existing in the spectrum, and I have no doubt that they would be seen in the spectrum of the Lime Ball light condensed by a polyzonal lens, and acted upon by thirty miles of atmosphere.

The absorptive action of the atmosphere shews itself in a less precise manner in the production of dark bands, whose limits

are not distinctly defined. A very remarkable narrow one, corresponding to one produced by the nitrous acid gas, is situated on the most refrangible side of C. Another very broad one lies on the most refrangible side of D, close to a sharp and broad band of yellow light, displayed by the general absorption of the corresponding part of the superimposed blue spectrum. There is also an imperfectly defined atmospheric action, corresponding to a group of lines where Dr Wollaston placed his line C.

This general description of the atmospheric lines, while it indicates the remarkable fact, that the same absorptive elements which exist in nitrous acid gas exist also in the atmospheres of the sun and of the earth, leads us to anticipate very interesting results from the examination of the spectra of the planets. Fraunhofer had observed in the spectra of Venus and Mars, some of the principal lines of the solar spectrum. This, indeed, is a necessary consequence of their being illuminated by the sun, for no change which the light of that luminary can undergo, is capable of replacing the rays which it has lost. But while we must find in the spectra of the planets and their satellites, all the defective lines in the solar spectrum, we may confidently look for others arising from the double transit of the sun's light through the atmospheres which surround them.

Allerly, *April* 12. 1833.

Abstract of a Paper accompanying a Suite of Volcanic Rocks from the Lipari Islands, presented to the Royal Society. By Robert Allan, Esq. F. R. S. E., M. G. S., &c.

(*Read 16th January* 1833.)

Having frequently had the pleasure of examining the very interesting and highly valuable collection of volcanic specimens brought from Iceland, and presented to the Royal Society by Sir George Mackenzie, I was gratified in an opportunity which was afforded me, at the close of 1830, of visiting the Lipari Islands, the only other great repository of obsidian in Europe, and of there collecting the suite of specimens which I have now the honour of presenting to the society. Unfortunately, it is far from complete as a geological series; for the difficulties occasioned by the tempestuous state of the weather, and the impossibility of getting to the more remote islands in a small boat during the month of December, limited my movements extremely, and forced me to confine my observations to the two principal islands of the group, viz. Lipari and Vulcano. I do not therefore pretend to give any geological account of these islands, or even of the two on which I landed. My object in collecting, and consequently also in presenting to the Society, this series of specimens, was to afford the means of comparing with each other the productions of the two great obsidian districts of Europe; those from Lipari, which are now on their table, with the Iceland suite already in their possession.

In so far as I remarked in the island of Lipari, the large and more continuous tracts of obsidian formed the lower strata, while those of a more pumaceous aspect appeared invariably at a higher level. The purest, blackest, and most beautiful specimens, however, are found in nodules in the upper portions of the pumice, where they occur in masses from two inches to as many feet in diameter. The two first specimens are from a locality of this description, occurring perhaps 1000 feet above the level of the sea, among the fine white stratified pumice of that portion of the island called Campo Bianco.

No. 1. is a specimen of the compact, conchoidal, black obsidian, for which, among mineralogists, the Lipari Islands have so long been a celebrated locality. It is translucent on the edges, presents a perfectly vitreous fracture, and is only found in boulders or masses imbedded among the pumice.

Some portions of this obsidian are less compact, and in No. 2. we remark indications of a more slaggy structure, with several white veins intersecting the black surface in a very beautiful manner.

The succeeding specimens of the series were collected in less elevated positions, from the obsidian *in situ*. They are all more or less impure; some of them exhibiting a very vitreous aspect, others a more earthy, and even a pumaceous appearance. None of them are so perfectly black and compact as the obsidian boulders which occur among the pumice, but they have all in some way or other a close analogy to them.

No. 3. deviates the least from the pure black obsidian of the two last. It is as nearly as possible the same substance, but is interspersed throughout with small white opake globules, which frequently appear stratified, as if formed parallel to the faces of the bed.

Nos. 4. and 5. are specimens of the stratified obsidian, which have more of an ash-grey appearance, and consist of alternate

layers of obsidian, and an earthy substance which appears to be compact pumice. No. 5. is an extremely beautiful example of this.

No. 6. The ash-grey coloured portion of this specimen is so perfectly vitreous, that doubts may be entertained whether it should properly be considered obsidian or pumice. The cross fracture resembles obsidian, while that parallel to the stratified arrangement exhibits a pumaceous structure. It is intimately blended with alternate layers of black obsidian, and has, on the whole, a very close resemblance to many of the slags from our glass-houses.

No. 7. is another specimen composed of alternate layers of black and ash-grey obsidian, so closely united as to afford one uniform mass, with a fine flat conchoidal fracture, and smooth vitreous aspect. This is a rare variety, and certainly the most beautiful I met with.

No. 8. again, is one of the commonest kinds which the island affords; its matrix is a black obsidian like that of No. 3, but so completely interspersed with the white globules, that it sometimes can scarce be recognised as that mineral. These globules are placed so much in parallel lines, as to give the rock the appearance of stratification.

No. 9. is another very beautiful specimen of the same, in which the globules or spheroids are greatly larger, and more irregular. Each of them is coated with a soft white silky substance consisting of silex, which appears to have a fibrous structure radiating towards the centre. A nodule of the same material occupies the middle of the cavity, but is so slightly attached, or rather occurs so often quite loose, that on breaking the rock it disengages itself, and falls out. When a portion of this specimen is held towards the light, the white globules appear quite opake, while the black matrix is as transparent as bottle-glass.

No. 10. is the same substance in a more compact state, the

globules imbedded in the black matrix being as hard, and almost as vitreous, as the obsidian itself. They present, in fact, a mineral resembling pearlstone, each of the spheroids being composed of a double series of rings, of two distinct hues, enclosing a central point, of a lighter colour; but equally, as in the last specimen, presenting a radiated structure. This is very similar to the appearances familiar to us in the productions of glass-furnaces.

No. 11. presents a variety of colour not often met with among the obsidians of these islands. It is of a dark liver-brown, streaked with black, and contains imbedded masses, which are of a white colour, and have a pumaceous texture.

No. 12. may be denominated pitchstone-lava. It has somewhat of the pearlstone structure, and presents a stratified aspect parallel to the faces of the bed. Its colour is dark ash-grey, mixed with some brownish tinges. It is devoid of the brilliant vitreous aspect of the obsidian, and is more nearly allied to pitchstone, than to any other volcanic product.

No. 13. This specimen was taken from a rock at some distance from the rest. It broke in slaty fragments, like some portions of the pitchstone from Arran. Its texture is vitreous throughout, its colour brown and mottled, bearing traces of the globular structure described in some of the preceding specimens; while on its surface there is an iridescent tarnish, peculiar, in so far as I remarked, to this variety.

The specimens from Nos. 14. to 23, form a suite of the pumice rocks, all occurring within the space of a few yards, at a quarry situated high up on the hill of Campo Bianco, the great deposit of this mineral at the northern extremity of the island. No. 14. is so light and spongy it floats on water like froth. Nos. 18. and 19. present the most delicate glassy texture, while No. 20. is the variety usually exported as an article of commerce. Its structure is somewhat fibrous, and its filaments have a peculiarly silky aspect. In No. 22. the pumice reassumes a charac-

ter more nearly allied to pitchstone-lava ; and in No. 23. it only occupies a small portion of the mass, presenting a compact vitreous texture, and a light ash-grey colour, exceedingly like a glass-house slag.

The next four are varieties of the compact pumice, which is used on the island as a building-stone,—a purpose for which, on account of its lightness and freedom from humidity, it is peculiarly well adapted.

Nos. 24. and 25. are from the eastern side of the island ; the latter being a portion of what some masons were excavating from the foundation of a house for the construction of its walls. No. 26. is much stratified, and was taken from Monte Gallina, at the southern extremity of the island, as was also No. 27, though greatly dissimilar in structure and appearance.

Nos. 28. and 29. form embedded masses in the Pumice of Campo Bianco, and were picked up along with the greater part of the foregoing. They are evidently of volcanic origin, but that they properly come under the denomination of "Pumice," admits of some doubt.

No. 30. is termed the *lava* of Monte Gallina. It has a somewhat pumaceous feel and appearance ; is much lighter and more friable than most lavas ; is stratified in a singular manner ; and contains numerous imbedded crystals of felspar.

No. 31. is another very peculiar rock, having little apparent relation to any of the preceding ; evidently of volcanic origin, however, and mottled with small cavities, which are coated with silex.

No. 32. On a small hill immediately above the town of Lipari, there occur, imbedded in clay or decomposed tuff, considerable masses of selenite, confusedly crystallized, stellated and fibrous, of which this is a specimen.

No. 33. Specular ir o-ore, occurring in cavities upon Monte Rosse, in which it has been sublimated by the action of heat.

Some of these crystals are extremely beautiful, branching out to the extent of several inches, and frequently presenting the most fantastical shapes.

The remainder of the suite are the productions of the island of Vulcano; most of them having been collected within and around the margin of its crater.

No. 34. is either the obsidian of the year 1775, which Dolomieu tells us flowed over the side of the crater in a stream; or that produced during the last eruption in 1786. I found it about half way up the mountain, generally in large broken masses, but in several places also distinctly forming very extensive surfaces. It is the blackest and most vitreous variety on the island, and, judging from the lengthened shape of the cavities interspersed throughout it, must have flowed quickly, and cooled rapidly.

No. 35. is the common variety of obsidian from this locality. It is neither so compact, so translucent, nor so vitreous as the Lipari species, and has much more the aspect of pitchstone, than of obsidian.

No. 36. This is an interesting piece, as exhibiting clearly the gradation between obsidian, pitchstone, and pumice. One portion of it is closely allied to the last specimen, while another has equally near relation to the next.

No. 37. The common pumice of the island of Vulcano, where, however, it is by no means of so fine a quality as in Lipari; and where it is never worked for any commercial or useful purpose.

No. 38. is a variety of the coarse pumic containing an imbedded mass of obsidian. The remaining specimens were all collected within the crater. With one of the men employed in gathering the sulphur, I descended by a rugged and circuitous path, but with comparative ease, about 400 feet into the smoking abyss; and examined its surface with perfect facility.

No. 39. is the kind of porcelain jasper peculiar to this locality, and occurring in great abundance inside the crater.

Nos. 40. to 45. are specimens of the different rocks found in the same situation. Some of them evidently decomposed, and much changed, by the action of heat and acidulous fumes.

Nos. 46. and 47. are specimens of the native sulphur, as it is collected and conveyed to the manufactory. The former is somewhat crystallized; while the latter presents a fibrous structure, and was taken in a hot state from the mouth of a fumarole. The minute white silky crystals occupying the interstices in the slaggy mass of No. 48. seem to be selenite.

No. 49. is the native alum from this island, disposed in veins of a fibrous structure, and having a shining silky appearance; and No. 50, is the beautiful white boracic acid, which forms in sublimation on the surface of the sulphur, to the depth of from half an inch to two inches; and of which variety the crater of Vulcano is the principal locality.

On the Colours of Natural Bodies. By Sir DAVID BREWSTER, K. H. LL. D. F. R. S. LOND. V. P. R. S. EDIN.

(*Read 22d December* 1833.)

THERE are few of the applications of optical science so universally interesting as that which has for its object the explanation of the colours of natural bodies. Sir ISAAC NEWTON was the first person who ventured to refer to one general principle all the variety of colours which are found in nature; and he has maintained his opinions on this subject with a confidence in their accuracy which seems to have confounded his adversaries: For while his analysis of light, the most perfect of all his labours, exposed him to the most harassing controversies, his theory of natural colours, the least perfect of his speculations, was allowed to pass without examination or censure.

During the century which has elapsed since the death of NEWTON this theory has been generally received and admired: In our own day it has been ingeniously defended, and beautifully illustrated, by M. BIOT; and, with few exceptions, it has been adopted by most of the distinguished philosophers of the present age.

The author of this theory has presented it under the two following propositions, one of which states the general cause of the phenomena, and the other the particular constitution of natural bodies on which their colours depend.

1. " Every body reflects the rays of its own colour more copiously than the rest, and from their excess or predominance in the reflected light, has its colour.

Fig. 1

SHEWING the COMPOSITION of the GREEN of the [redacted] THIRD *ORDER*

Fig. 2

SHEWING the SPECTRUM produced by the GREEN JUICES of PLANTS.

Lizars sculp.

2. "The transparent parts of bodies, according to their several sizes, reflect rays of one colour, and transmit those of another, on the same ground, that thin plates or bubbles do reflect or transmit those rays."

In estimating the truth of the theory which is contained in these two propositions, I do not intend to enter into any examination of the postulates, facts, and reasonings, on which it is founded. The object of the following paper is to analyze one leading phenomenon of colour, and to apply this analysis as an *experimentum crucis*, in determining the true origin of all colours similarly produced.

The colour which I have chosen for this purpose is the *Green colour of the vegetable world*, and I have made this selection for the following reasons:—

1. The green colour of plants is the one most prevalent in nature.

2. It is the colour of which Sir ISAAC NEWTON has most distinctly described the nature and composition.

3. Its true composition is almost identically the same in all the variety of plants in which it appears.

Sir ISAAC NEWTON has described this colour in the following manner:—

"There may be good *greens* of the fourth order, but the purest are of the *third*. And of this order the *green* of all vegetables seems to be, partly by reason of the intenseness of their colours, and partly because, when they wither, some of them turn to a *greenish-yellow*, and others to a more perfect *yellow* or orange, or perhaps to *red*, passing first through all the aforesaid intermediate colours. Which changes seem to be effected by the exhaling of the moisture which may leave the tinging corpuscles more dense, and something augmented by the accretion of the oily and earthy part of that moisture. Now the *green*, without doubt, is of the same order with those colours into which it changeth, because the changes are gradual, and those colours, though usu-

ally not very full, yet are often too full and lively to be of the fourth order."

Having thus determined that the green colour of vegetables must, according to this theory, be a *green of the third order*, we must inquire into its composition. Sir Isaac has himself stated, that the green of the third order "is principally constituted of original green, but not without a mixture of some blue and yellow." In point of fact, it consists of all the rays of the green space, with the least refrangible rays of the blue space, and the most refrangible rays of the *yellow* space, and it does not contain a single ray of *indigo* or *violet*, nor a single ray of *orange* or *red light*. This is its real composition, whether we deduce it from the theory of periodical colours, or obtain it by direct analysis with the prism.

In order to discover the true composition of the green colour of plants, we may analyze the light which they reflect or transmit; but the best method is to extract the green colouring matter by means of alcohol, and to examine the action of the tinging corpuscles when suspended in that fluid. For this purpose I have used the leaves of the common Laurel, *Prunus Lauro-cerasus*, as a type of this class of colours. The leaves are torn into small shreds and put into absolute alcohol, and the fine green fluid which is thus obtained is either placed in a hollow prism with a large refracting angle, so as to exhibit its composition in its own spectrum, or the light transmitted through the fluid may be analyzed by a fine prism, or the spectrum produced by such a prism may be viewed through a portion of the fluid bounded by parallel surfaces. By whichsoever of these methods the experiment is made, we shall observe a spectrum of the most beautiful kind. In place of seeing the *green* space with a portion of *blue* on one side and *yellow* on the other, as the Newtonian theory would lead us to expect, we perceive a spectrum divided into several coloured bands of unequal breadths, and having their colours greatly changed by absorption.

At a certain thickness of the green fluid there are three *red* bands. By increasing the thickness, the violet and blue spaces are absorbed, and the two inner red bands. An absorption then begins near the middle of the green space, and after destroying the more refrangible portion of that space, three bands are left; viz. *one* faint band of the extreme *red*, *one* band almost *white*, corresponding with the most luminous spectrum, and one *green* band contiguous to the white one.

The general effect of the absorption now described is shewn rudely in the second of the accompanying figures, the first of which represents the effect which would have been produced had the colour been a phenomenon of thin plates, and a *green* of the third order. The simple inspection of Figure 2. affords the most unquestionable evidence that the green colour under consideration is neither the green of the third order nor of any other order of periodical colours, and that in its general character, as well as in the character of its component bands, it has no resemblance whatever to any colour produced by the action of thin plates.

In applying this mode of examination to the green colours of other plants, I have found them to have invariably the same composition. In the following list of plants of various characters, I have given those in which I have made the experiments with most care. Excepting where it is otherwise mentioned, the green fluid was extracted from the leaves:

White Lilac.	White Jasmine.	Aucuba Japonica.
—— Convolvulus.	Thuja occidentalis.	Juniperus communis.
Tulip-Tree.	Arbutus Unedo.	Camellia Japonica.
Mignionette.	Hemerocallis flava.	The green berries of the Convallaria multiflora.
Common Pea.	Celastrus scandens.	The green berries of the Asparagus officinalis.
Daphne Cneorum.	Viburnum Tinus.	
Virginian Raspberry.	Prunus Lusitanica.	

When the green fluid obtained from these plants has stood for *three* or *four* days, it loses its high green colour, and becomes

of an olive-green, which grows more and more of a brownish-yellow, till it becomes almost colourless. During these various changes, the specific action of the fluid upon the spectrum changes also; but neither the change of colour nor the change of action have any relation whatever to the effects of an increase or decrease of thickness in the tinging corpuscles, by which Sir Isaac Newton explains the changes which take place in the colour of leaves. When the fluid has become almost colourless like water, it still exercises a powerful action upon the middle of the *red* space, and a faint, but still perceptible action, at two points of the *green* band. This curious fact may lead us to expect that transparent media may yet be discovered, which shall absorb different parts of the spectrum, while they themselves are perfectly colourless. This effect of course cannot take place unless the rays absorbed compose white light.

In the course of these experiments, I observed a very remarkable phenomenon, which at first sight appeared to be somewhat favourable to the Newtonian theory. In making a strong beam of the sun's light pass through the green fluid, I was surprised to observe that its colour was a brilliant *red*, complementary to the *green*. By making the ray pass through greater thicknesses in succession, it became first *orange* and then *yellow* and *yellowish-green*, and it would undoubtedly have become blue, if it had been transmitted through a greater thickness of fluid. This mode of producing a spectrum by reflexion from the particles of a fluid, exhibits the phenomenon of opalescence in a very interesting form. Had the green fluid shewn the same colour at all thicknesses, or had it absorbed only the red rays, the opalescent beam would have been *red* throughout the whole of its path: but as the different colours are absorbed in different proportions, and, in the present case, in the order of their refrangibility, excepting the blue and violet, the colour of the intromitted beam must vary from red to greenish-yellow, as these colours are successively taken out of it.

The analysis of this experiment is very interesting, but as this is not the place to pursue it, I shall only remark, that I have observed the same phenomenon in various other fluids of different colours, that it occurs almost always in vegetable solutions, and almost never in chemical ones, or in coloured glasses; and that it is a phenomenon of opalescence or imperfect transparency. One of the finest examples of it which I have met with may be seen by transmitting a strong pencil of solar light through certain cubes of bluish fluor-spar. The brilliant blue colour of the intromitted pencil is singularly beautiful.

According to the Newtonian theory of colours, the green of plants is of the same order as the *yellow* and *orange* into which it is changed when it withers, in consequence of an increased density, or an enlargement of size in the tinging corpuscles. In order to put this opinion to the test of experiment, I extracted the yellow juice from the brilliant yellow leaves of the *common laurel*. This fluid becomes of a deep red at great thicknesses. It attacks the spectrum powerfully towards the extremity of the green space, a place where it is not touched by the green fluid. It then absorbs the *yellow* and *violet*, leaving a bright green, and converting the blue into violet. At greater thicknesses, the violet disappears, and the absorption advances gradually to the red.

For the purpose of varying the experiment, I extracted the juice of the leaves of the privet, which become of a deep black violet when they wither, a colour which has not the most remote resemblance to any periodical tint. The fluid was a deep red colour, much deeper than that of the darkest port-wine. It attacked the red part of the spectrum near the line B of FRAUNHOFER, at the same place that the green juice attacked it, leaving *two red* bands, the innermost of which vanished at an increased thickness. It then absorbed the violet and blue spaces generally; and having obliterated the middle of the green space, the absorption advanced to the orange rays at D.

Now, in both these experiments, the action of the colouring matter of the decayed leaves is decidedly different from that of the green juice, and there is no appearance whatever of the tints having any such relation as that which subsists between adjacent colours of the same order.

From facts like these, which it is impossible to misinterpret, we are entitled to conclude, that the green colour of plants, whether we examine it in its original verdure, or in its decaying tints, has no relation to the colours of thin plates.

I have submitted to the same mode of examination nearly *one hundred and fifty* coloured media, consisting of fluids extracted from the petals, the leaves, the seeds, and the rhind of plants,—the different substances used in dyeing,—coloured glasses and minerals,—coloured artificial salts,—and different coloured gases; and in all these cases I have obtained results which lead to the same conclusion. I have analyzed, too, the *blue* colour of the sky, to which the Newtonian theory has been thought peculiarly applicable, but instead of finding it a *blue* of the first order, in which the extreme red and the extreme violet rays are deficient, while the rest of the spectrum was untouched, I found that it was defective in rays, adjacent to some of the fixed lines of Fraunhofer, and that the absorptive action of our atmosphere widened, as it were, these lines. Hence it is obvious, that there are elements in our atmosphere which exercise a specific action upon rays of definite refrangibility, and that this, in some of these rays, is identical with that which is exercised over them by the atmosphere of the sun. I have obtained analogous results in analyzing the *yellow*, *orange*, *red*, and *purple* light, which is reflected from the clouds at sunset; but it is impossible to convey any correct idea of the composition of these colours, without a reference to the fixed lines of the spectrum, of which we at present possess no distinct nomenclature.

I may mention, however, this general fact, that in the various specific actions exercised upon light by solids, fluids, vapours,

and gases, the points at which the spectrum is attacked are generally coincident with the deficient lines of Fraunhofer; and particularly with those which are common to the light of the sun, and that of some of the fixed stars. Hence it appears, that these rays or lines are weak parts of the spectrum, or the parts of white light which have the greatest affinity for those elements of matter, which, while they enter into the composition of sublunary bodies, exist also in the atmospheres of the central luminaries of other systems.

From the preceding experiments, it is impossible to resist the conclusion, that the second and leading proposition of Newton's theory of colours is incompatible with the actual phenomena; and we may demonstrate the incorrectness of the first proposition by simply stating the fact, that there are *red*, *yellow*, *green*, and *blue* media, which are absolutely incapable of reflecting or transmitting certain definite rays of the same colour with themselves.

The true cause of the colours of natural bodies may be thus stated: When light enters any body, and is either reflected or transmitted to the eye, a certain portion of it, of various refrangibilities, is lost within the body; and the colour of the body, which evidently arises from the loss of part of the intromitted light, is that which is composed of all the rays which are not lost; or, what is the same thing, the colour of the body is that which, when combined with that of all the rays which are lost, compose the original light. Whether the lost rays are reflected or detained by a specific affinity for the material atoms of the body, has not been rigorously demonstrated. In some cases of opalescence, they are either partly or wholly reflected; but it seems almost certain, that in all transparent bodies, and in that great variety of substances in which no reflected tints can be seen, the rays are detained by absorption.

PROCEEDINGS

OF THE

EXTRAORDINARY GENERAL MEETINGS,

AND

LIST OF MEMBERS ELECTED AT THE ORDINARY MEETINGS,

SINCE APRIL 4. 1831.

PROCEEDINGS, &c.

November 28. 1831.

At an Extraordinary General Meeting held this day, Dr Hope, V. P. in the Chair, the following Office-bearers were elected for the ensuing year:

Sir Walter Scott, Bart. President.

The Hon. Lord Glenlee,
The Hon. Lord Newton,
Dr T. C. Hope,
Professor Russell,
Sir Thomas Makdougal Brisbane,
Dr Brewster,
} Vice-Presidents.

John Robison, Esq. General Secretary.

Dr Gregory,
Dr Christison,
} Secretaries to the Ordinary Meetings.

Thomas Allan, Esq. Treasurer.

James Skene, Esq. Curator of the Museum.

John Stark, Esq. Assistant Curator.

COUNCILLORS.

Sir David Milne.
Sir George S. Mackenzie, Bart.
Dr Duncan.
Professor Wallace.
Dr Greville.
James Jardine, Esq.
Dr Hibbert.
Sir Thomas Dick Lauder, Bart.
J. D. Forbes, Esq.
General Straton.
Alexander Adie, Esq.
James Wilson, Esq.

The following Committee was appointed in terms of Law XXI.:—

John Gardiner Kinnear, Esq.
Patrick Neill, Esq.
John Robison, Esq.

ORDINARY MEETINGS.

January 2. 1832.

In pursuance of notice given on the 4th April 1831, Dr J. C. Gregory moved, " That the 13th Law shall be so far altered, that ballots for the admission of members may take place at any ordinary meeting of the Society during the session."

This motion having been put from the Chair, and having been seconded by Sir George Ballingall, was carried unanimously.

January 16. 1832.

MEMBERS ELECTED.

ORDINARY.

John Sligo, Esq. of Carmyle.
Major Alexander Anderson.
James Dunlop, Esq. Astronom. N. S. Wales.
James F. W. Johnston, Esq. A. M.

February 20. 1832.

MEMBERS ELECTED.

HONORARY.

Sir John F. W. Herschel.

March 5. 1832.

MEMBER ELECTED.

ORDINARY.

Robert Morrieson, Esq. Hon. E. I. C. Civil Service.

In announcing the death of the Hon. Lord Newton, one of the Vice-Presidents, Lord Meadowbank pronounced an eloquent eulogium upon the character and acquirements of the distinguished member whose loss the Society has to deplore, and concluded by proposing that a committee should be appointed to draw up a short memorial, expressive of the feelings of the Society on this melancholy occasion, to be inserted in the minutes.

Lord Meadowbank, Mr Allan, and Mr Skene were appointed, with the Secretary, a committee for this purpose.

April 2. 1832.

MEMBERS ELECTED.

ORDINARY.

MONTGOMERY ROBERTSON, M. D.	WILLIAM DYCE, Esq. A. M.

November 26. 1832.

At a General Meeting held this day, Dr HOPE, Vice-President, in the Chair, the following Office-bearers were elected for the ensuing year:

Sir THOMAS MAKDOUGAL BRISBANE, K. C. B. President.

The Hon. Lord GLENLEE,
Dr T. C. HOPE,
Professor RUSSELL,
Sir DAVID BREWSTER, } Vice-Presidents.

JOHN ROBISON, General Secretary.

Dr J. C. GREGORY,
Dr CHRISTISON, } Secretaries to the Ordinary Meetings.

THOMAS ALLAN, Esq. Treasurer.

JAMES SKENE, Esq. Curator of the Museum.

JOHN STARK, Esq. Assistant Curator.

COUNCILLORS.

Dr GREVILLE.	ALEXANDER ADIE, Esq.
JAMES JARDINE, Esq.	JAMES WILSON, Esq.
Dr HIBBERT.	The Hon. MOUNTSTUART ELPHINSTONE.
Sir THOMAS DICK LAUDER, Bart.	Sir HENRY JARDINE.
J. D. FORBES, Esq.	Dr ABERCROMBIE.
Major-Gen. Sir JOSEPH STRATON.	ARTHUR CONNELL, Esq.

The Committee appointed at last Annual Meeting for auditing the Treasurer's accounts was continued.

ORDINARY MEETINGS.

December 3. 1832.

Before the commencement of the Ordinary Business of the Meeting, Lord MEADOWBANK gave notice, that, at the next Meeting, he intended to make a motion to the following effect, viz.

"That the Royal Society shall direct a sum of money to be subscribed from their Funds for the Monument proposed to be erected to the memory of their late President Sir WALTER SCOTT, Bart.

"That it shall be remitted to the Council to consider and report as to the amount of the sum to be subscribed."

December 17. 1832.

At the General Meeting of this evening, Lord MEADOWBANK made the motion of which notice was given at the last; the motion was seconded by ROBERT STEVENSON, Esq. Thereafter, the following amendment was moved by the Lord Chief-Commissioner, the Right Hon. WILLIAM ADAM, and was seconded by Dr MACLAGAN, viz.

"That a tribute of respect be paid by the Royal Society to the memory of Sir WALTER SCOTT, and that the Council consider and report in what manner this can be done most appropriately."

On the amendment being put from the Chair, the Society divided, when there appeared 23 members in favour of it, and 15 for the original motion.

After the above numbers were taken, one member intimated, that it was owing to a misapprehension that he was included in the minority.

January 7. 1833.

At this meeting, it was intimated that the Council had appointed JAMES D. FORBES, Esq. as one of the Secretaries to the Ordinary Meetings, in the room of the late Dr GREGORY.

January 21. 1833.

MEMBERS ELECTED.

ORDINARY.

His Grace the Duke of BUCCLEUCH.
ALEXANDER EARLE MONTEITH, Esq. Advocate.
GEORGE MEIKLE, Esq. Surgeon Hon. E. I. C. Service.

February 4. 1833.

Previous to the Ordinary Business of the evening, the Council made the following Report:

"In pursuance of the remit made by the General Meeting of the 17th December, to consider and report in what manner a tribute of respect can be most appropriately paid to the memory of Sir WALTER SCOTT, the Council beg leave to recommend, that a request should be made, in the name of the Society, to the friends of Sir WALTER SCOTT, to communicate a Biographical notice or Eloge of this distinguished person, to be inserted in the next Volume of the Society's Transactions.

"The Council, on considering the inadequacy of the Funds of the Society for the due accomplishment of the direct objects of its institution, regret that they cannot recommend the appropriation of any sum from the corporate Funds towards the expense of a Monument; but they humbly suggest, that an Extraordinary Fund should be raised for this purpose, by voluntary Contributions from the Members, the amount of which, when collected, shall be paid by the Treasurer, *in name of the Society*, towards the Memorial now contemplated."

The Report having been read, Lord MEADOWBANK moved that a Special Meeting be called to take it into consideration; which motion having been duly seconded, was carried *nemine contradicente;* after which it was agreed the Meeting should be held on the 8th instant, at three o'clock P. M., and the Secretary was directed to issue notices accordingly.

SPECIAL GENERAL MEETING.

February 8. 1833.

Lieutenant-General Sir THOMAS MAKDOUGALL BRISBANE in the Chair.

The Minute of the Meeting of the 4th instant was read, after which it was moved by Dr MACLAGAN, and seconded by Dr BORTHWICK, that the Report of the Council be adopted.

Lord MEADOWBANK addressed the Meeting at considerable length, and proposed several plans for disposing of the question in a different way from that proposed by the Council, but concluded without making any motion; after which, the Lord Chief-Commissioner spoke, and having stated that his object was to obtain such a Resolution as should be acceptable to all concerned, and be unanimously agreed to by the Society, he proposed, in confirmation of the Report of the Council,

"That, as an appropriate Tribute of respect to the memory of Sir WALTER SCOTT, late President of the Society, it be resolved, that a Biographical Notice and Eloge of this distinguished person be inserted in the next Volume of the Transactions; and that Professor WILSON be requested to prepare such Notice and Eloge.

"That, as a further tribute of respect to the memory of Sir WALTER SCOTT, the Society do subscribe the sum of £50 to the Monument intended to be erected in this City; but, as the present state of the Finances of the Society does not admit of a grant from its corporate Funds, to make good the proposed contribution, that it be raised by voluntary Subscription of the Members, to be placed in the hands of the Treasurer: That the contribution, when completed, shall be paid, in name of the Society, to the Treasurer of the Subscriptions for the Monument, with directions to enter it as the Subscription of the Royal Society of Edinburgh to the Monument to be erected in commemoration of Sir WALTER SCOTT: That

The Right Hon. the Lord PRESIDENT,
The Right Hon. the Lord JUSTICE-CLERK,
The Right Hon. WILLIAM ADAM, Lord Chief-Commissioner,
The Hon. Lord MEADOWBANK,
Sir FRANCIS WALKER DRUMMOND, Bart.
JOHN HOPE, Esq. Dean of Faculty,
GEORGE FORBES, Esq.
GEORGE MACPHERSON GRANT, Esq., and
Rear-Admiral ADAM,

Fellows of this Society, together with the Council, be requested to see this Resolution carried into effect, and do report to the Society at their meeting on the first Monday in March."

The Lord Chief-Commissioner concluded by suggesting, that the amount of the Subscription from the individuals should be restricted to £1.

This Resolution being generally approved of, was adopted by the Meeting *nemine contradicente.*

February 18. 1833.

The Chairman announced that arrangements had now been made for having Abstracts of the Papers read at each meeting prepared, to be read along with the Minutes at the subsequent one.

March 4. 1833.

MEMBERS ELECTED.

ORDINARY.

A. T. J. GWYNNE, Esq. DAVID CRAIGIE, M. D.

The Chairman announced that the Subscription by the Society to the Monument of Sir WALTER SCOTT now amounted to £60. It was agreed to, on the motion of Dr HOPE, that the Subscription should remain open till the first meeting in April.

April 1. 1833.

MEMBER ELECTED.

ORDINARY.

GEORGE BUCHANAN, Esq. Civil Engineer.

The President announced that the Subscription by the Society to the Monument for Sir WALTER SCOTT, now amounting to £70, would be paid over to the Treasurer of the Committee for that object.

April 15. 1833.

MEMBER ELECTED.

ORDINARY.

Sir JOHN STUART FORBES, Bart.

May 6. 1833.

MEMBERS ELECTED.

ORDINARY.

JOHN DUNLOP, Esq. Advocate.
ALEXANDER HAMILTON, Esq. W. S.
The Right Hon. Lord GREENOCK.
J. W. TURNER, Esq. Professor of Surgery in the University of Edinburgh.

The President announced that the Council had awarded the Keith Prize to Mr THOMAS GRAHAM of Glasgow, for his discovery of the Law of Diffusion of the Gases.

LIST OF THE PRESENT ORDINARY MEMBERS IN THE ORDER OF THEIR ELECTION.

HIS MAJESTY THE KING, PATRON.

Date of Election.

James Hamilton senior, M. D. *Edinburgh.*
Sir William Miller, Baronet, Lord Glenlee.
James Russell, Esq.

The above Gentlemen were Members of the Edinburgh Philosophical Society.

1783. Honourable Baron Hume.

The above Gentleman was associated with the Members of the Philosophical Society at the institution of the Royal Society in 1783.

THE FOLLOWING MEMBERS WERE REGULARLY ELECTED.

1784. Rev. Archibald Alison, LL. B. *Edinburgh.*
1787. James Home, M. D. *Professor of the Practice of Physic.*
1778. Thomas Charles Hope, M. D. F. R. S. Lond. *Professor of Chemistry.*
Right Honourable Charles Hope, *Lord President of the Court of Session.*
1793. Sir Alexander Muir Mackenzie, Bart. *of Delvin.*
1795. The Very Reverend Dr George Husband Baird, *Principal of the University.*
1796. The Honourable Baron Sir Patrick Murray, Bart.
1798. Alexander Monro, M. D. *Professor of Anatomy, &c.*
Right Honourable Sir John Sinclair, Bart.
1799. Thomas Macknight, D. D.
Honourable Lord Robertson.
Sir George S. Mackenzie, Baronet, F. R. S. Lond.
Robert Jameson, Esq. *Professor of Natural History.*
1802. Colonel D. Robertson Macdonald.
1803. John Jamieson, D. D.
Thomas Telford, Esq. *Civil Engineer.*
Reverend Dr Andrew Brown, *Professor of Rhetoric.*

Date of Election.

1804. William Wallace, Esq. *Professor of Mathematics.*

1805. Thomas Thomson, M.D. F. R. S. Lond. *Professor of Chemistry, Glasgow.*

1806. Robert Ferguson, Esq. *of Raith,* F. R. S. Lond.

George Dunbar, Esq. *Professor of Greek.*

1807. Sir James Montgomery, Baronet, *of Stanhope.*

John Campbell, Esq. *of Carbrook.*

Thomas Thomson, Esq. *Advocate.*

William Fraser Tytler, Esq. *Advocate.*

1808. James Wardrop, Esq. *Surgeon Extraordinary to his Majesty.*

Sir David Brewster, Knight, LL.D. F. R. S. Lond.

1811. Sir Charles Bell, *Surgeon, London.*

Reverend Andrew Stewart, M. D. *Erskine.*

David Ritchie, D. D. *Professor of Logic.*

Major-General Sir Thomas Makdougal Brisbane, K. C. B.

1812. General Dyce.

John Thomson, M. D. *Professor of General Pathology, Edinburgh.*

James Jardine, Esq. *Civil Engineer.*

Captain Basil Hall, R. N. F. R. S. Lond.

J. G. Children, Esq. F. R. S. Lond.

Alexander Gillespie, Esq. *Surgeon, Edinburgh.*

W. A. Caddell, F. R. S. Lond.

Macvey Napier, Esq. F. R. S. Lond. *Professor of Conveyancing.*

James Pillans, Esq. *Professor of Humanity.*

Sir George Clerk, Baronet, F. R. S. Lond.

Daniel Ellis, Esq. *Edinburgh.*

1813. William Somerville, M. D. F. R. S. *London.*

Henry Davidson, M. D. *Edinburgh.*

1814. Sir Henry Jardine, *King's Remembrancer in Exchequer.*

Patrick Neill, Esq. *Secretary to the Wernerian and Horticultural Societies.*

Right Honourable Lord Viscount Arbuthnot.

Reverend John Thomson, *Duddingston.*

John Fleming, D. D. *Clackmannan.*

John Cheyne, M. D. *Dublin.*

Lieutenant-Colonel Tytler, *Edinburgh.*

Alexander Brunton, D. D. *Professor of Oriental Languages.*

Professor George Glennie, *Marischall College, Aberdeen.*

1815. Robert Stevenson, Esq. *Civil Engineer.*

Sir Thomas Dick Lauder, Baronet, *of Fountainhall.*

Henry Home Drummond, Esq. *of Blair-Drummond.*

Charles Granville Stewart Menteath, Esq. *of Closeburn.*

Date of Election.

1815. William Thomas Brande, Esq. F. R. S. Lond. and *Professor of Chemistry in the Royal Institution.*

1816. Colonel Thomas Colby, F. R. S. Lond. *Royal Engineers.*
Leonard Horner, Esq. F. R. S. Lond.
Henry Colebrooke, Esq. *Director of the Asiatic Society of Great Britain.*
George Cooke, D. D. *Professor of Moral Philosophy, St Andrew's.*
Right Hon. William Adam, *Lord Chief-Commissioner.*
Honourable Lord Fullerton.
Thomas Jackson, LL. D. *Professor of Natural Philosophy, St Andrew's.*
John Robison, Esq. *Edinburgh.*
Hugh Murray, Esq. *Edinburgh.*

1817. Right Hon. Earl of Wemyss and March.
John Wilson, Esq. *Professor of Moral Philosophy.*
Honourable Lord Meadowbank.
James Hamilton Dickson, M. D. *Clifton.*
William P. Alison, M. D. *Professor of the Theory of Physic.*
James Skene, Esq. *of Rubislaw.*
Reverend Robert Morehead, *Northumberland.*
Robert Bald, Esq. *Civil Engineer.*
Thomas Sivright, Esq. *of Meggetland.*

1818. William Richardson, M. D. *Harrowgate.*
Right Honourable Lord Napier.
Harry William Carter, M. D. *Oxford.*
Patrick Miller, M. D. *Exeter.*
John Craig, Esq. *Edinburgh.*
John Watson, M. D.
John Hope, Esq. *Dean of Faculty.*
William Ferguson, M. D. *Windsor.*
Sir William Hamilton, Baronet, *Professor of Civil History.*

1819. Right Honourable Lord John Campbell, F. R. S. Lond. and M. R. I. A.
Dr Shoolbred, *Calcutta.*
Patrick Murray, Esq. *of Simprim.*
James Muttlebury, M. D. *Bath.*
Thomas Stewart Traill, M. D. *Professor of Medical Jurisprudence.*
Mr Alexander Adie, *Optician, Edinburgh.*
William Couper, M. D. *Glasgow.*
Marshall Hall, M. D. *Nottingham.*
John Borthwick, Esq. *Advocate.*
Richard Phillips, Esq. F. R. S. Lond.
Reverend William Scoresby.

Date of Election

1819. George Forbes, Esq. *Edinburgh.*
1820. James Hunter, Esq. *of Thurston.*
Right Honourable David Boyle, *Lord Justice-Clerk.*
James Keith, Esq. *Surgeon, Edinburgh.*
Right Honourable Sir Samuel Shepherd.
James Nairne, W. S. *Edinburgh.*
John Colquhoun, Esq. *Advocate.*
Lieutenant-Colonel M. Stewart.
Charles Babbage, Esq. F. R. S. Lond.
Thomas Guthrie Wright, Esq. *Auditor of the Court of Session.*
Sir John F. W. Herschel, F. R. S. Lond.
Adam Anderson, A. M. *Rector of the Perth Academy.*
John Schank More, Esq. *Advocate.*
George Augustus Borthwick, M. D. *Edinburgh.*
Robert Dundas, Esq. *of Arniston.*
Samuel Hibbert, M. D. *Edinburgh.*
Robert Haldane, D. D. *Principal of St Mary's College, St Andrew's.*
Sir John Meade, M. D. *Weymouth.*
Dr William Macdonald *of Ballyshear.*
Sir John Hall, Baronet, *of Dunglass.*
Sir John Hay, Baronet, *of Smithfield and Hayston,* M. P.
Sir George Ballingall, M. D. *Professor of Military Surgery.*
1821. Major-General Sir Joseph Straton, K. C. B.
Robert Graham, M. D. *Professor of Botany.*
A. N. Macleod, Esq. *of Harris.*
Sir James M. Riddell, Baronet, *of Ardnamurchan.*
Archibald Bell, Esq. *Advocate.*
John Clerk Maxwell, Esq. *Advocate.*
John H. Wishart, Esq. *Surgeon, Edinburgh.*
John Lizars, Esq. *Professor of Surgery to the Royal College of Surgeons, Edin.*
John Cay, Esq. *Advocate.*
Robert Kay Greville, LL. D. *Edinburgh.*
Robert Hamilton, M. D. *Edinburgh.*
Sir Archibald Campbell, Baronet.
Sir David Milne, K. C. B.
Colonel Mair, *Deputy-Governor of Fort George.*
A. R. Carson, Esq. LL. D. *Rector of the High School.*
James Buchan, M. D. *Edinburgh.*
James Tytler, Esq. *of Woodhouselee,* W. S.
1822. Francis Chantry, Esq. F. R. S. Lond.

Date of Election.

1822. Edward Troughton, Esq. F. R. S. Lond.
James Smith, Esq. *of Jordanhill.*
William Bonar, Esq. *Edinburgh.*
Rev. H. Parr Hamilton, *Cambridge.*
Captain J. D. Boswall, R. N. *of Wardie.*
George A. Walker Arnott, Esq. *Advocate.*
Rev. John Lee, M. D. *Edinburgh.*
Sir James South, F. R. S. Lond.
Lieutenant-Colonel Martin Whyte, *Edinburgh.*
Walter Frederick Campbell, Esq. *of Shawfield*, M. P.
George Joseph Bell, Esq. *Professor of Scots Law.*
Dr William Dyce, *Aberdeen.*
W. C. Trevelyan, Esq. *Wallington.*
Sir Robert Abercromby, Baronet, *of Birkenbog.*
Thomas Shortt, M. D. *Edinburgh.*
Dr Wallich, *Calcutta.*

1823. The Right Honourable Sir George Warrender, Baronet, *of Lochend.*
John Russell, Esq. W. S. *Edinburgh.*
John Shaw Stewart, Esq. *Advocate.*
Alexander Hamilton, M. D. *Edinburgh.*
Right Honourable Sir William Rae, Baronet, *of St Catherine's.*
Sir Robert Dundas, Baronet, *of Beechwood.*
William Cadell, Esq. *of Cockenzie.*
Sir William Knighton, Baronet.
Sir Edward Ffrench Bromhead, Baronet, A. M. F. R S. Lond. *Thurlsby Hall.*
Sir James Stuart, Baronet, *of Allanbank.*
Sir Andrew Halliday, M. D.
John Bonar, Esq. *of Kimmerghame.*
Captain Thomas David Stuart, *of the Hon. East India Company's Service.*
Andrew Fyfe, M. D. *Lecturer on Chemistry, Edinburgh.*
Robert Bell, Esq. *Advocate, Procurator for the Church of Scotland.*
Captain Norwich Duff, R. N.
Warren Hastings Anderson, Esq.
Alexander Thomson, Esq. *of Banchory, Advocate.*
Liscombe John Curtis, Esq. *Ingsdon House, Devonshire.*
Robert Knox, M. D. *Lecturer on Anatomy, Edinburgh.*
Robert Christison, M. D. *Professor of Materia Medica.*
John Gordon, Esq. *of Cairnbulg.*

1824. George Harvey, Esq. F. R. S. Lond. *Plymouth.*
Dr Lawson Whalley, *Lancaster.*

Date of Election.

1824. William Bell, Esq. W. S. *Edinburgh.*
Alexander Wilson Philip, M. D. *London.*
James Hamilton junior, M. D. *Professor of Midwifery in the University of Edinburgh.*
Admiral Adam, R. N.
Robert E. Grant, *Professor of Comparative Anatomy in the London University.*
Claud Russell, Esq. *Accountant, Edinburgh.*
Rev. Dr William Muir, *one of the Ministers of Edinburgh.*
W. H. Playfair, Esq. *Architect, Edinburgh.*
John Argyle Robertson, Esq. *Surgeon, Edinburgh.*
James Pillans, Esq. *Edinburgh.*
James Walker, Esq. *Civil Engineer.*
William Newbigging, Esq. *Surgeon, Edinburgh.*
William Wood, Esq. *Surgeon, Edinburgh.*
John Campbell, M. D. *Edinburgh.*
George Anderson, Esq. *Rector of the Inverness Academy.*

1825. Rev. John Williams, *Rector of the Edinburgh Academy.*
W. Preston Lauder, M. D. *London.*
Right Honourable Lord Ruthven.
Major Leith Hay *of Rannes.*
Edward Turner, M. D. *Professor of Chemistry in the London University.*
Dr Reid Clanny, *Sunderland.*
Sir John Archibald Stewart, Baronet, *of Grandtully.*
Sir William Jardine, Baronet, *of Applegarth.*
Alexander Wood, Esq. *Advocate.*
Rev. Dionysius Lardner, *London University.*

1826. George Macpherson Grant, Esq. *of Ballindalloch.*
William Renny, Esq. W. S. *Solicitor of Stamps.*
Elias Cathcart, Esq. *Advocate.*
Andrew Clephane, Esq. *Advocate.*
Rev. George Coventry.
Sir David Hunter Blair, Baronet.
George Moir, Esq. *Advocate.*
John Stark, Esq. *Edinburgh.*
Dr Macwhirter, *Edinburgh.*

1827. James Weddell, Esq. R. N.
John Gardiner Kinnear, Esq. *Edinburgh.*
William Burn, Esq. *Edinburgh.*
James Russell junior, M. D. *Edinburgh.*

Date of Election.

1827. Prideaux John Selby, Esq. *Twizel House, Northumberland.*
Henry Thornton Maire Witham, Esq. *of Lartington.*
John Reddie, Esq. LL. D. *Edinburgh.*
Rev. Dr Robert Gordon, *one of the Ministers of Edinburgh.*
James Wilson, Esq. *Edinburgh.*
Rev. Edward Bannerman Ramsay, A. B. *of St John's College, Cambridge.*
James Walker, D. D. *of St John's College, Cambridge.*
Alexander Copland Hutchison, Esq. *Surgeon, London.*
George Swinton, Esq. *Dean House, Edinburgh.*

1828. Sir Francis Walker Drummond, Baronet, *of Hawthornden.*
Erskine Douglas Sandford, Esq. *Advocate.*
David Maclagan, M. D. *Edinburgh.*
Captain Maxwell, *K. D. Guards.*
John Forster, Esq. *Architect, Liverpool.*
Thomas Graham, Esq. A. M. *Glasgow.*
Thomas Hamilton, Esq. *Edinburgh.*
David Milne, Esq. *Advocate.*
Dr Manson, *Nottingham.*
William Burn Callender, Esq.

1829. Andrew Skene, Esq. *Advocate.*
A. Colyar, Esq.
William Gibson Craig, Esq. *Advocate.*
Charles Ferguson, Esq. *Advocate.*
James Ewing, LL. D. M. P. *Glasgow.*
Duncan Macneill, Esq. *Sheriff-depute of Perthshire.*
Rev. John Sinclair, A. M. *Pembroke College, Oxford.*
Arthur Connell, Esq. *Advocate.*
James Hope Vere, Esq. *of Craigiehall.*
Bindon Blood, Esq. M. R. I. A.
James Walker, Esq. W. S.
William Bald, Esq. M. R. I. A.
Whitelaw Ainslie, M. D. M. R. A. S.

1830. Colonel Pitman, *Hon. East India Company's Service.*
J. T. Gibson Craig, Esq. W. S.
Archibald Alison, Esq. *Advocate.*
Honourable Mountstuart Elphinstone.
James Syme, Esq. *Professor of Clinical Surgery.*
Thomas Brown, Esq. *of Langfine.*
James L'Amy, Esq. *Sheriff-depute of Forfarshire.*
Thomas Barnes, M.D. *Carlisle.*

Date of Election.

1831. James D. Forbes, Esq. *Professor of Natural Philosophy.*
Right Honourable James Abercromby, M. P.
John Abercrombie, M. D. *Edinburgh, First Physician to his Majesty in Scotland.*
Donald Smith, Esq.
Captain Samuel Brown, R. N.
O. Tyndal Bruce, Esq *of Falkland.*
David Boswell Reid, M. D. *Lecturer on Chemistry, Edinburgh.*
T. S. Davies, Esq. A. M. *Bath.*

1832. John Sligo, Esq. *of Carmyle.*
Major Alexander Anderson.
James Dunlop, Esq. *Astronomer New South Wales.*
James F. W. Johnston, Esq. A M. *Professor of Chemistry in the University of Durham.*
Anthony Dickson, Esq. *Pres. Bengal Medical Board.*
William Gregory, M. D. *Edinburgh.*
Robert Allan, Esq. *Advocate.*
Robert Morrieson, Esq. *Hon. E. I. C. Civil Service.*
Montgomery Robertson, M. D.
William Dyce, Esq. A. M.

1833. Captain Milne, R. N.
Alexander Earle Monteith, Esq. *Advocate.*
George Meikle, Esq. *Surgeon H. E. I. C. Service.*
His Grace the Duke of Buccleuch.
A. T. J. Gwynne, Esq.
David Craigie, M. D. *Edinburgh.*
George Buchanan, Esq. *Civil Engineer.*
Sir John Stuart Forbes, Baronet, *of Pitsligo.*
John Dunlop, Esq. *Advocate.*
Alexander Hamilton, Esq. W. S.
Right Honourable Lord Greenock.
J. W. Turner, Esq. *Professor of Surgery.*

LIST OF NON-RESIDENT AND FOREIGN MEMBERS,

ELECTED UNDER THE OLD LAWS.

Sir Gilbert Blane, M. D. F. R. S. Lond.
Right Honourable Sir Robert Liston, Bart.
M. Le Chevalier, *Paris.*
Dr S. L. Mitchell, *New York.*
Right Honourable Lord Wallace.
John Gillies, LL. D. *Historiographer to his Majesty.*
M. P. Prevost, *Geneva.*
Rev. Bishop Gleig, *Stirling.*
Charles Hatchett, Esq. F. R. S. Lond.
Sir Henry Steuart, Baronet, *of Allanton.*
Sir William Blizzard, M. D. F. R. S. Lond.
Thomas Blizzard, Esq.
Sir William Ouseley, Baronet.
Sir James Macgrigor, M.D.
Richard Griffiths, Esq. *Civil Engineer.*

LIST OF HONORARY AND FOREIGN MEMBERS,

ELECTED UNDER THE NEW LAWS.

CLASS OF HONORARY MEMBERS, LIMITED TO TWENTY-ONE.

M. Le Baron Humboldt, *Member of the Institute of France.*

M. Gay Lussac, *Member of the Institute of France.*

M. Biot, *Member of the Institute of France.*

His Majesty the King of Belgium.

His Royal Highness the Archduke Maximilian.

The above Members were elected before the New Class of Foreign Members was established.

His Imperial Highness the Archduke John of Austria.

M. Le Chevalier Joseph Hammer.

Rev. Dr Brinkley, F. R. S. Lond. *and President of the Royal Irish Academy.*

Robert Brown, Esq. F. R. S. Lond. &c. &c.

Jacob Berzelius, M. D. F. R. S. Lond. *Professor of Chemistry, Stockholm.*

Davies Gilbert, Esq. M. P. F. R. S. Lond.

His Royal Highness the Duke of Sussex, *President of the Royal Society of London.*

CLASS OF FOREIGN MEMBERS, LIMITED TO THIRTY-SIX.

M. Poisson, *Member of the Institute of France.*

M. le Baron de Prony, *Member of the Institute of France.*

M. Brochant, *Member of the Institute of France.*

Baron Leopold Von Buch, *Berlin.*

M. Gauss, *Professor of Mathematics, Göttingen.*

M. Blumenbach, *Professor of Natural History, Göttingen.*

M. J. C. L. Simonde de Sismondi.

Baron Degerando.

Baron Krusenstern, *Member of the Academy of Sciences at St Petersburg.*

M. Oersted, *Secretary to the Royal Society of Denmark.*

M. Ampere, *Member of the Institute of France.*

M. Schumacher, *Professor of Astronomy at Copenhagen.*

M. Mohs, *Professor of Mineralogy at Freyberg.*

David Hosack, M. D. F. R. S. *New York.*

Nathaniel Bowditch, Esq. *Salem Massachussets.*

M. le Baron Larrey, *Member of the Institute of France.*

Sir Henry Bernstein, *Professor of Oriental Literature in the University of Berlin.*

M. De Candolle, *Geneva.*

Dr Olbers, *Bremen.*

M. Frederick Munter, *Bishop of Zealand.*

M. le Baron Dupin, *Member of the Institute of France.*

M. Brongniart, *Member of the Institute of France.*

The Chevalier Bürg, *Vienna.*

M. Bessel, *Konigsberg.*

M. Thenard, *Member of the Institute of France.*

M. Haidinger, *Vienna.*

M. Mitscherlich, *Professor of Chemistry in the University of Berlin.*

M. Gustavus Rose, *Professor of Mineralogy in the University of Berlin.*

G. Moll, *Professor of Natural Philosophy in the University of Utrecht.*

M. Stromeyer, *Professor of Chemistry in the University of Göttingen.*

M. Hausmann, *Professor of Mineralogy in the University of Göttingen.*

John James Audubon, Esq. M. W. S.

Le Chevalier Bouvard, F. R. S. Lond. *Member of the Institute of France.*

L. A. Necker, *Honorary Professor of Mineralogy and Geology at Geneva, Foreign Member of the Geological Society of London.*

LIST OF DECEASED MEMBERS, AND OF MEMBERS RESIGNED,

FROM 1831 TO 1833.

(N. B.—This List is necessarily incomplete.)

Baron Cuvier, *Secretary to the Institute of France.*
Right Honourable Earl of Dundonald.
M. Goethe.
Count Volta, *Como.*
Rev. Walter Fisher, *Cranston.*
Sir James Hall, Baronet, F. R. S. Lond.
Honourable Lord Eldin.
James Hare, M. D. *late of Calcutta.*
Robert Hamilton, Esq. *Professor of Public Law.*
Andrew Duncan, M.D. *Professor of Materia Medica.*
Gilbert Innes, Esq. *of Stow.*
Sir Walter Scott, Baronet, *of Abbotsford.*
Honourable Lord Newton.
Thomas Allan, Esq. F. R. S. Lond.
George Bell, Esq. *Surgeon.*
Sir John Leslie, *Professor of Natural Philosophy in the University of Edinburgh.*
Sir James Macintosh, F. R. S. Lond.
Honourable Baron Clerk Rattray.
James Crawfurd Gregory, M. D.
William Crosbie Mair, M. D.
John Aytoun, Esq. *of Inchdarnie.*
Gilbert Laing Meason, Esq. *of Lindertis.*
Rev. W. H. Marriot.
Sir Charles Giesecké, *Professor of Mineralogy to the Dublin Society.*
Alexander Nimmo, Esq. *Civil Engineer.*
Andrew Berry, M. D. *Edinburgh.*
Sir Alexander Keith, *Knight Marischal.*

Sir William Macleod Bannatyne, Bart.
M. Oriani, *Milan.*
M. le Chevalier Legendre, *Member of the Institute of France.*
Major James Alston *of Auchenard.*

RESIGNATIONS.

Sir William G. Cumming Gordon, Baronet.
Patrick Fraser Tytler, Esq. Advocate.

LIST OF DONATIONS.

(Continued from Vol. XI. p. 545.)

December 5. 1831.

DONATIONS.	DONORS.
The Edinburgh Journal of Natural and Geographical Science for May.	The Editor.
Charges against the President and Council of the Royal Society by Sir James South.	The Author.
American Journal of Science and Arts, Vol. xix. No. 2.	Prof. Silliman.
The Edinburgh Journal of Natural and Geographical Science for June.	The Editor.
Mémoires de l'Académie Royale des Sciences de l'Institut de France, pour l'année 1826, tom. x.	Acad. of Sciences.
Rudiments of the Primary Forces of Gravity, Magnetism, and Electricity, in their agency on the Heavenly Bodies. By P. Murphy, Esq.	The Author.
Statement of circumstances connected with the late Election for the Presidency of the Royal Society.	Ditto.
Annual Report of the Yorkshire Philosophical Society for 1830, and List of Members and Contributors for 1831.	The Society.
American Journal of Science and Art, Vol. xx. No. 1.	Prof. Silliman.
Flora Batava, Nos. 88, and 89.	King of Holland.
Abhandlungen der Akademie der Wissenschaften zu Berlin, 1827.	Acad. Sc. Berlin.
Transactions of the Royal Irish Academy, Vol. xvi. part 2.	The Academy.
American Journal of Science and Arts, Vol. xx. No. 2.	Prof. Silliman.
Transactions of the Royal Society of London, 1828, part ii.; 1829, part ii., and 1831, part i.	Royal Society.
Essays on various subjects of Medical Science, Vol. iii. By David Hosack, M. D. F.R.S. L. & E. Professor of the Theory and Practice of Physic in Rutger's Medical College, New York.	The Author.
Bijdragen tot Natuurkundige Wetenschaffen, Verzameld door H. C. von Hall, W. Vrolik, en G. J. Mulder, 1830, Nos. 1, 2, 3, 4.	The Editors.
Cambridge Philosophical Society Transactions, Vol. iii. part iii., and Vol. iv. part i.	The Society.
Catalogue of the Library of the Royal College of Surgeons in London.	The College.
Catalogue of the Contents of the Museum of the Royal College of Surgeons in London, parts iii. iv. v.	Ditto.

DONATIONS.	DONORS.
Catalogue of the Hunterian Collection in the Museum of the Royal College of Surgeons in London, parts i. ii.	The College.
Geography of Western Asia, by the late Major Rennell, 2 vols. 8vo, and Atlas, fol.	Mrs Jane Rodd.
The Book of Analysis, or a New Method of Experience. By Tweedie John Todd, M. D.	The Author.
Observations on Cholera, as it appeared in Port-Glasgow. By Dr John Marshall.	The Author.
Transactions of the Linnean Society of London, Vol. xvi. part ii.	The Society.
Catalogue of several hundred Manuscript Works collected by Sir William Ouseley.	Sir W. Ouseley.
History of the Maritime Wars of the Turks. Translated from the Turkish of Haji Khalifeh by James Mitchell, Esq.	The Translator.
Report of the Scarborough Philosophical Society for 1830.	The Society.
Eighth Report of the Whitby Literary and Philosophical Society, 1831.	Ditto.
Original Letters from James Gregorie, Professsor of Mathematics in the University of St Andrew's, to the Rev. Colin Campbell, Minister of Ardchattan, Argyleshire.	Society of Antiquaries.
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December 19.

Transactions of the Royal Society of London for 1831, part ii.	The Society.
Astronomische Nachrichten, Nos. 196–212.	Prof. Schumacher.

January 2. 1832.

Bulletin de la Société Géologique, tom. i.	The Society.
Herman von Meyer's Beiträge zur Petrefactenkunde.	The Author.
Atlas zu der Reise im Nordlichen Afrika, von Eduard Rüppell, Hefts 16–26.	Frankfort Institution.
Positions Géographiques de l'Oby depuis Tobolsk, jusqu' à la Mer glaciale, corrigées par Adolphe Erman.	The Author.
Memoirs of the Wernerian Natural History Society, Vol. vi.	The Society.
Account of the First Meeting of the British Association for the advancement of Science, held at York in September 1831. By James F. W. Johnston, Esq. A. M.	The Author.

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Cholera, its Nature, Cause, Treatment, and Prevention clearly and concisely explained. By Charles Searle, Esq. of the Hon. E. I. C. Madras Establishment.	The Author.
Cambridge Philosophical Society Transactions, Vol. i. part ii.; Vol. ii. parts i. ii., and Vol. iii. part ii.	The Society.
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Tableau des Successions des Terrains et des Roches de l'Ecorce de la Terre. Par M. Alexandre Brongniart.	The Author.
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Astronomische Nachrichten, Nos. 221–232. inclusive.	M. Bessel.
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List of the Fellows of the Royal Society of London for 1832–33.	Ditto.
Addresses delivered at the Anniversary Meetings of the Royal Society of London on November 30. 1831, and November 30. 1832.	Ditto.
Memoirs of the Royal Astronomical Society.	The Society.
Astronomische Nachrichten, No. 233–238.	M. Schumacher.
Nova Acta Physico-Medica Academiæ Cæsareæ Leopoldino-Carolinæ Naturæ Curiosorum, Vols. x. xi. xii. xiii. xiv. and xv. part i.	Imper. Acad. of Bonn.
Transactions of the Linnean Society of London, Vol. xvi. part iii.	The Society.
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ociété Géologique de France, Tome iii. feuilles 1–5.	

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